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SPECTRA OF WEAK LUMINESCENCES

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I. THE FLUORESCENCE SPECTRUM OF FLUOR-SPAR

Probably no substance has been more often used for the demonstration and study of fluorescence than the mineral from which the name of the phenomenon was taken. Stokes, who suggested this name,[1] examined the fluorescence of fluorites from various sources, and in his paper "On the Long Spectrum of Electric Light"[2] he gives the results obtained from certain specimens from Alston Moor in Cumberland, England. These crystals were quite pale by transmitted light, of a somewhat brownish-purple color, possessing a strong bluish fluorescence, and phosphorescing with the same blue color for a long time after the removal of the exciting source. Besides this blue fluorescence, which has ever since been described as the one characteristic of the substance, Stokes mentions in the same paper the appearance of a *red* fluorescence of the same crystals under the exciting influence of the spark between certain metallic terminals. That the red color is caused by ultra-violet light Stokes showed by the fact that it disappears completely when a sheet of thin glass is inserted between the exciting source and the crystal, and that it is even weakened by the introduction of a considerable thickness of quartz, as by the use of a quartz condensing lens. He noticed also

[1] *Phil. Trans.*, 1852. [2] *Ibid.*, 1862.

that the red fluorescence is confined to certain strata which are characterized by a deeper color than the remainder of the crystal, and that the light exciting the red color seems to be absorbed by the crystal itself before reaching a great depth, the fluorescence being less intense in the deeper strata. Stokes did not examine the *spectrum* of these fluorescences, but it seems probable that, if spectroscopy had been a slightly more advanced science at that time, he would have carried the investigation farther and discovered the facts about the spectra of these two fluorescences of fluorite, the blue and the red, which are the subject of this paper, and which have been completely overlooked by everyone who has up to the present time worked on the subject of fluorescence.

Fluor-spar is remarkable for the number of different spectra which may be obtained from it under various conditions. Becquerel[1] gives data on the fluorescent color and the spectrum emitted in the phosphoroscope by seven varieties of the mineral of various colors: yellow, green, blue, and purple. Excited by condensed sunlight, a series of bands appeared in the spectrum of the emitted phosphorescence, including a blue-green one which determines the fluorescence color of the mineral under ordinary circumstances of excitation, and others which appear at various rates of rotation of the phosphoroscope and which combine to determine the phosphorescence color at these various rates. Besides these different spectra and the red fluorescence already referred to, which is not excited by daylight, fluor-spar from various sources may give at least three different spectra of thermo-luminescence. Finally, as will be seen later, the spectrum of fluorescence of certain crystals may vary decidedly and sharply with the exciting source.

While examining a series of fluorites from Weardale, England, I was led by the weakness of the emitted light to photograph the fluorescence spectra for the purpose of more accurate comparison. In some of these photographed spectra the "orange band," noticed by many of the observers of the spectrum of fluor-spar, was resolved into what appeared to be narrow bands and *sharp lines*. The exciting source in these cases happened to be the spark between magnesium terminals, and the photographs showed plainly at least four sharp

[1] *La Lumière*, Vol. I, p. 360.

lines not in the comparison spectrum of the spark. It seemed at first possible that light had in some way entered from without at these points, and that an optical or instrumental error was responsible for the remarkable appearance; but a few simple qualitative tests showed that this was not the case, and that the yellow, orange, and red of the fluorescence of this particular crystal of fluorite, as produced under these conditions, is in fact composed in large part of comparatively sharp lines.

The crystals examined were all from Weardale, England, and their physical properties follow:

a) By transmitted light:

No. 1. Colorless. Contained many bubbles and irregularities.

No. 2. Bright green.

No. 3. Yellow. About the color of amber.

Nos. 4–8. Five crystals of various shades of purple and brownish-purple.

b) Fluorescence color (unresolved) and phosphorescence:

No. 1. Weak fluorescence of the usual bluish color.

No. 2. Very strong fluorescence of same color as above.

No. 3. Weak fluorescence. Color same as above.

Nos. 4–8. Strong fluorescence of about equal strength in all. Color the usual bluish.

The phosphorescence color of all of the crystals was of approximately the same color as the fluorescence and varied in strength in about the same way. They all showed long-continued phosphorescence.

c) With direct-vision spectroscope and spark between iron terminals the observed spectra were as follows:

No. 1. Gave weakly the broad blue band ordinarily observed.

No. 2. Gave the blue band very strongly, and in addition to it strong maxima in the green and orange. The two latter maxima were almost completely cut out by introducing a piece of microscopic cover-glass between the spark and the crystal, but not by the introduction of a quartz plate several millimeters in thickness. The blue band is only slightly reduced by the glass and not at all by the quartz plate.

No. 3. Gave the blue band, the green band weakly, the orange band strongly. The two latter were cut out by glass as in No. 2. The

fluorescence spectrum from this crystal is nearly continuous and has approximately the same distribution as that from the filament of an incandescent lamp.

Nos. 4–8. Gave the blue band strongly, the green band weakly, and, in addition to these, a number of apparently sharp lines and bands in the yellow, orange, and red. These were cut out by glass, with the exception of the blue band, but were only slightly weakened by a thick layer of quartz.

It is my intention to return to the green and yellow crystals as soon as possible, but the more evident sharpness of the lines in the spectrum from the purple crystals made their examination easier, and the present paper is devoted to them.

The preliminary qualitative results were obtained by visual work with a direct-vision spectroscope, and they may be briefly stated as follows:

1. The fluorescence spectrum from these purple Weardale fluorites contains many sharp lines, comparable in sharpness with the lines in the exciting source—the spark in air between metallic terminals. These lines disappear almost completely when a thin film of glass is interposed in the path of the exciting light. Microscopic cover-glass (0.17 mm) allows sufficient of the light to pass to cause an exceedingly weak appearance of the lines. A plate of quartz several millimeters in thickness has no appreciable absorbing effect.

Lines belonging to the spectrum of the exciting source, which have reached the spectroscope by reflection on cleavage surfaces and other irregularities inside the crystal, are of course not in the least affected by the introduction of either glass or quartz in the path of the exciting light. The glass plate affords, therefore, an easy method of deciding what lines belong to the fluorescence spectrum and what lines have been accidentally introduced from the spark.

2. The spectrum varies when the exciting source is changed. Even this first preliminary examination showed that the strongest lines in the fluorescence excited by several metals have the same wave-length within the limit of accuracy of the method. But some metals evidently excite the appearance of many more lines than others, and the above statement therefore describes the facts.

3. The spectrum varies from crystal to crystal with the same

exciting source. The entire series of purple fluorites from Weardale showed very similar spectra from the same spark, but later and more exact measurements on them showed distinct differences. Crystals of different color (Nos. 2 and 3, for example) give spectra which are entirely different from those obtained from the purple specimens.

4. No sharp lines appear when sunlight is used as the exciting source. This statement is limited to the experiments which I have carried out, and which do not by any means represent the accuracy desirable. It is very possible that better methods may show the sharp lines in this case also.

5. Only one line has been observed when the arc between carbon poles is used as exciting source.

The small direct-vision spectroscope with which the visual work was done has also been used in the photography of the fluorescence spectra. In spite of its small size, it is well adapted for work of this sort, as the aperture is large (about $\frac{1}{4}$), and good photographs for qualitative examination could be obtained with exposures of less than an hour, using a fairly broad slit-width. For the later more accurate work an instrument was designed with special reference to the photography of spectra from very weak sources of light. This is a spectrograph of the ordinary form, having old Voigtländer portrait lenses of aperture $\frac{1}{3}$ for collimator and objective, and a prism of sufficient size to take the large beam transmitted by these lenses (about 5 cm clear aperture). This type of instrument (which might be provided with old-fashioned portrait lenses of still greater aperture) probably represents very nearly the limit in light-giving power. There are many disadvantages in such an instrument. The dispersion is, of course, very small, and the cone of light is so large that there is very little depth of focus, but the resolving power is great, and under good conditions of working the two sodium lines are perfectly separated, although they are distant only 0.02 mm. Owing to the large cone, the curvature of the field becomes noticeable even over the space of an inch, which is about the length of the spectrum from λ 4000 to λ 7000, and it was found necessary to put the center of the spectrum out of focus in order that the part from λ 5000 to λ 6500, which contains most of the lines under investigation, should be sharp.

The light from the spark was condensed by means of a concave

speculum mirror of about 17 cm aperture and 13 cm focal length. This was mounted for adjustment in all directions and gave good illumination. The spark was from an induction coil for 12 cm spark-length, driven by the 110-volt alternating commercial circuit and provided with a Wehnelt interrupter. The latter was made of large size in order to avoid undue heating during the prolonged exposures. The crystal under examination was mounted on a stand in front of the slit, with screens to prevent the entrance of light not coming from the crystal, which was illuminated in the usual manner from the side by the large cone from the concave mirror. The spark was placed behind the crystal, with an opaque screen between.

Using this prism spectrograph of large aperture, good photographs were obtainable with exposures of from two to eight hours, the time varying with the metal used as exciting source. As most of the sharp lines in the fluorescence lie in the yellow and red part of the spectrum, it was necessary to work with plates sensitive to this region. After some unsuccessful work with various sensitizers, Cramer "Trichromatic" plates were used. They give good results and a fair sensitiveness as far as λ 6500.

In order to test the real *sharpness* of these apparently sharp lines under higher dispersion, a few of the strongest lines in the fluorescence spectrum were photographed with a large aperture concave grating of 163 cm radius and about 2500 lines to the centimeter. With this instrument measurable plates for a few strong lines were obtained with an exposure of six hours, using as exciting source a metal giving a bright fluorescence. A number of the lines in these particular spectra seem to be as sharp as any of the lines in the exciting spark spectrum. The strong line at λ 5732.5, which was the first sharp line observed, was measured on photographs made with the grating with two different widths of slit, and it appears to be as sharp as, or sharper than, any line in the exciting source, which was magnesium. Another sharp line, produced in certain crystals when iron is the exciting source, has been measured with the grating and appears to be equally sharp. On the plates made with the prism spectrograph there are about twenty lines which are as sharp as these two, while many others, not so sharp, are what would be called exceptionally sharp lines to form part of a *fluorescence* spectrum.

In the spectrum produced by certain metals on some of the crystals there are no broad, diffuse portions in the region between λ 5000 and λ 6500, the spectrum containing only lines and narrow bands. Some of the other spectra contain bands which have every appearance of being resolvable into many fine lines with higher dispersion. They run up toward definite sharp heads in a most tantalizing way. The exposure necessary to get these bands on a photograph with the grating would run up into days, even with a slit of some width.

There have been measured in the spectra from these purple fluorites from Weardale some 200 lines and bands, lying between λ 4700 and λ 6400. A number of strong lines have been observed farther toward the red, but no visual wave-length measurements have as yet been made on them. The dispersion of the spectrograph is so small, especially out in the yellow and red end of the spectrum, that the measured wave-lengths given in the following tables have an accuracy not greater than 2 to 3 tenth-meters. It is therefore not possible to decide many questions of great importance about the identity of lines occurring near one another in various spectra. No attempt has been made to decide these questions from the data at hand, and the wave-lengths have been given as they were in the laboratory notes taken when they were measured.

The tables given below contain data on the fluorescence spectra of two crystals of purple fluor-spar under the exciting influence of several metallic sparks. One of these is a natural crystal, and the other has had its faces polished to remove the roughness and irregularities which reflect light from the spark into the slit of the spectrograph. As will be seen from the tables, these two crystals give very different spectra of fluorescence with the same exciting source, the natural crystal showing lines peculiar to itself and the polished one lines representative of the other four crystals examined—Nos. 5, 6, 7, and 8. Photographs of the fluorescent spectrum from these four crystals taken before they were polished show no differences from the later ones taken after polishing. The latter are comparatively free from spark lines which have been accidently reflected into the slit, and they have therefore been used for the more accurate measurements.

EXCITING SOURCE, MAGNESIUM SPARK

Crystal No. 4	Crystal No. 5 (Fig. 1)
5283 (5) sharp	5285 (5) sharp
5337 }	
5373 |	
5405 |	
5435 |	
5470 } Rather diffuse maxima.	
5500 |	
5580 |	
5612 |	
5670 }	
5713 (3) sharp	5715 (2) sharp
5733 (100) sharp	5736 (100) sharp
5770	
5800 (10) sharp	5803 (10) sharp
5838 broad line	
5850 } (2) sharp	
5885 } unresolved band	
5915 } diffuse maxima	
5953 }	
6055 (4) quite sharp	
6067 (2) sharp	
6109 (2) sharp	
	6140 (10) sharp
6195 } diffuse maxima	
6210 }	
6250 (3) sharp	
6350 (5) sharp	

The exposure for the two crystals was the same—about four hours—but the diffuse lines which have come out so strongly in crystal No. 4 do not appear in measurable intensity in the crystal No. 5 with this exposure. There are faint indications of the bands in the latter, but they are not resolved or measurable.

As may be seen in the plate (Fig. 1), the spectrum of magnesium contains several strong sharp lines, including one of especial strength at λ 5733–λ 5736. The spectrum also contains a large number of weaker lines and bands which do not appear on the plate, but which have been measured and entered in the table of wave-lengths.

The spectra produced in different crystals by magnesium as exciting source do not appear to differ very noticeably in the strong lines within the limit of accuracy of the measurements. In the spectrum from crystal No. 5 there appears one strong sharp line at λ 6140 which does not appear in the fluorescence of the other crystal.

The effect of introducing inductance into the secondary circuit of

the spark is to lower the general strength of the exciting light, and therefore to greatly lengthen the exposure. Although the air lines appear to be completely cut out by the inductance used, the strong lines appear with the same sharpness and the same relative intensities as without the inductance (see Fig. 2). The exposure was insufficient to bring out the weaker lines or bands. It would seem that the air lines have nothing to do with the appearance of the strong lines in the case of magnesium, and the same proof has been obtained for other metals used as exciting source.

EXCITING SOURCE, IRON SPARK

Crystal No. 4 (Fig. 5)		Crystal No. 5 (Fig. 3)	
4747	diffuse lines	5256	diffuse lines.
5168		5287	
5236		5351	
5270		5416	sharp edge of band
5336		5425	
5374		5447	
5400		5481	(3) sharp
5434		5513	
5468	(3) sharp	5553	sharp end of band
5510	unresolved double?	5582	edge of band
5534		5619	(5) maxima in band
5574 to 5612	band	5672 to 5696	
5667	diffuse lines	5715	(20) sharp
5712		5727	(5) sharp
5732		5737	(20) sharp
5767		5777	diffuse lines
5807		5811	
5838		5851	(5) quite sharp
5870		5890	(3) quite sharp
5913		5922	(3) quite sharp
		6070	(15) sharp
6059	(5) sharp	6240	(3) sharp
6200	(3) sharp		

From the five purple crystals examined there have been obtained two entirely different spectra of fluorescence. These are to be divided into the spectrum from crystal No. 4, and that from the other crystals. Crystal No. 4 gives a spectrum composed of a large number of diffuse but quite narrow lines, with a few sharp strong lines, while the other crystals all give the same spectrum, containing a number of strong sharp lines and an entirely different arrangement of the weaker lines and bands from that in crystal No. 4.

The introduction of inductance into the secondary circuit has no effect on the position or relative strength of the strong lines. Since the intensity of the light from the spark is far less reduced in iron than in magnesium, the exposure required to photograph the spectrum is not very much longer than without the inductance.

In the plate, Fig. 3 shows the appearance of the fluorescence spectrum from the crystal No. 5 and Fig. 5 the spectrum from crystal No. 4. Fig. 4 shows the spectrum produced when inductance is introduced into the discharge circuit, the crystal in this case being No. 5.

EXCITING SOURCE, CADMIUM SPARK

Crystal No. 4 (Fig. 7)	Crystal No. 5 (Fig. 6)
5394, 5428, 5465, 5498 diffuse lines	5403 (1) sharp
	5408 (2) sharp
	5420 (10) sharp
5527 band head?	
5571, 5598 band	5453, 5468, 5483 underlying band below head
5696 diffuse toward red	
5759 diffuse	
5815, 5832 rather sharp lines	
5865 to 5902 unresolved lines?	
	5508, 5516, 5535, 5553 underlying band below head
6048 (10) sharp	
6250 (3) sharp	
	5568 diffuse
	5588, 5612, 5618 head
	5711 (20) sharp
	5723 (10) sharp
	5737 (7) sharp
	5770 diffuse
	5843 unresolved?
	6030 (15) sharp

The same differences between the spectra from the two crystals appear when cadmium is used as exciting source as have been already noticed under magnesium and iron. Fig. 6 gives the appearance of the spectrum from the crystal No. 5 and Fig. 7 that for the crystal No. 4. There are several very sharp and strong lines in the spectrum

PLATE VII

1. *Mg* Spark
Crystal No. 5

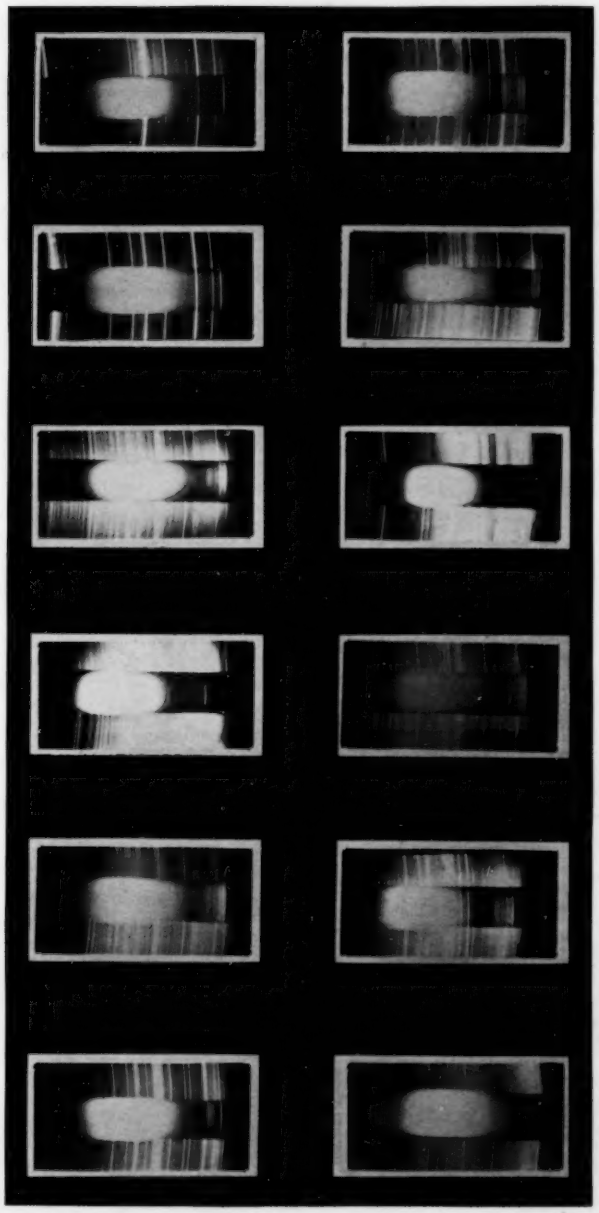
2. *Mg* Spark
No. 5. Inductance

3. *Fe* Spark
Crystal No. 5

4. *Fe* Spark
No. 5. Inductance

5. *Fe* Spark
Crystal No. 4

6. *Cd* Spark
Crystal No. 5



7. *Cd* Spark
Crystal No. 4

8. *Al* Spark
Crystal No. 5

9. *Al* Spark
No. 5. Inductance

10. *Al* Spark
Crystal No. 4

11. *Zn* Spark
Crystal No. 4

12. Arc

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shown in Fig. 6, notably the triplet λ 5711–λ 5737. The lines of this triplet are of the same order of sharpness as the lines of the comparison spectrum.

EXCITING SOURCE, ALUMINIUM SPARK

Crystal No. 4 (Fig. 10)	Crystal No. 5 (Fig. 8)
4757	4753 (3) sharp
4931	4914 (2) sharp
4961	5340 (1) sharp
5021	5378 (2) sharp
5260	5408 (2) sharp
5289	
5318 } rather diffuse lines	5442
5350	5451 } underlying band below
5381	5498
5420	5543
5450	
5490	5570
5532	5604 } underlying band below
	5617
5589	5658 } underlying band below
to } unresolved band	5672
5636	
5679	5710 (20) sharp
to } unresolved band	
5732 } (5) sharp	5720 (8) sharp
to	
5780	
to } unresolved band	5732 (3) sharp
5953	5750
	5767 } underlying band below
6081 (5) sharp	5774
6100 (3) sharp	5810
	5833 } underlying band below
	5842
	5867 } underlying band below
	5905
	6020 (3) sharp

Aluminium as exciting source gives for different crystals differences in the fluorescence spectra of the same sort as those already observed in the preceding metals. Crystal No. 4 gives a spectrum containing more diffuse lines, and crystal No. 5 shows lines of greater strength and sharpness.

It will be noticed that the group at λ 5710–λ 5732 agrees nearly with groups in the spectra produced by *Mg*, *Cd*, and *Fe*, with a slight shift in wave-lengths as compared with the last two metals. I have repeated the measurements on all of these spectra without convincing

myself that this shift is due to errors of measurement, but the accuracy in this part of the spectrum is so small that the possibility of such an error has not been eliminated.

Fig. 8 shows the appearance of the spectrum from crystal No. 5 and Fig. 10 that for crystal No. 4. Fig. 9 shows the effect of introducing inductance into the spark-gap, the crystal being one with polished faces. No difference whatever seems to be introduced and the conclusion is that the air lines do not play any important rôle in the excitation of the fluorescence.

EXCITING SOURCE, ZINC SPARK

Crystal No. 4 (Fig. 11)

5237 (3) sharp
5326 (2) sharp
5361 (3) sharp
5398 (10) sharp
5430
5460
5485 to 5528 unresolved band
5570
5598 appears to consist of several lines
5670 (3) sharp
5696 diffuse
5718 (2) sharp
5751 diffuse
5802 (3) sharp
5832 (10) sharp
5850 to 5885 unresolved
6045 diffuse

Fig. 11 shows the appearance of the fluorescence of this crystal under the exciting influence of the spark between zinc terminals. There appear under these circumstances a large number of sharp lines, more than this crystal shows for any other metal.

EXCITING SOURCE, MERCURY SPARK

Crystal No. 4

4926, 4962, 5010, 5058, 5115, 5159, 5225 diffuse lines
5482, 5533 diffuse lines
5610
5678 to 5963 unresolved

There are no sharp lines in this fluorescence spectrum.

EXCITING SOURCE, TIN SPARK

Crystal No. 4	Crystal No. 5
5733 (2) sharp	5720 (2) sharp
5835 to 5885 unresolved	5743 (2) sharp
6046	5760 (2) sharp

Tin as exciting source gives rise only to a weak fluorescence. The differences between the two crystals are as plainly marked as in the cases already considered.

The spark between lead terminals gives no lines of intensity sufficient to permit of photography or measurement, and the arc between ordinary carbon terminals gave only one line at λ 5733, the line appearing with both crystals (Fig. 12).

In all of the spectra examined, the blue band, broad and diffuse, is present, extending from about λ 4000 to λ 5000. The following diffuse bands underlying the lines given in the tables, have also been observed:

Iron: band from λ 5530 to λ 6000.
Cadmium: band from λ 5330 to λ 5980.
Zinc: band from λ 5320 to λ 5940.
Tin: band from λ 5460 to λ 6110.

Comparison of sharp lines in crystals under excitation by various metals:

CRYSTAL NO. 4

Mg	*Fe*	*Cd*	*Al*	*Zn*	*Sn*	Arc
				5237		
5283						
				5326		
				5361		
				5398		
	5468					
				5670		
5715				5718		
5733			5732		5733	5733
5800				5802		
				5832		
6055	6059	6048				
6067						
			6081			
			6100			
6109						
	6200					
6250		6250				
6350						

The table below contains the wave-lengths of the sharp lines of the previous tables. It will be seen that there are a few lines which appear to be common to several exciting metals. The most evident example is the line near λ 5735. This is a strong sharp line in the following spectra: from crystal No. 4 with *Mg*, *Al*, *Sn*, and the arc

CRYSTAL NO. 5

Mg	*Fe*	*Cd*	*Al*	*Zn*	*Sn*	Arc
			4753			
			4914			
5285						
			5340			
			5378			
		5403				
		5408	5408			
		5420				
	5481					
	5619					
5715	5715	5711	5710			
		5723	5720		5720	
	5727					
5736	5737	5737	5732			5733
					5743	
					5760	
5803						
	5851					
	5890					
	5922					
			6020			
		6030				
	6070					
6140						
	6240					

as exciting source; from crystal No. 5 with *Mg*, *Fe*, *Cd*, *Al*, and the arc as exciting source. The differences in the measured wave-lengths are about of the order of the error in this part of the spectrum. Other coincidences are: crystal No. 4: λλ 5715–5718 in *Mg* and *Zn*, λλ 5800–5802 in the same metals, λλ 6055–6048 in *Mg* and *Cd*, λ 6250 in the same metals. In crystal No. 5 the strong lines at λλ 5710–5715 in *Mg*, *Fe*, *Cd*, *Al*, and λλ 5720–5727 in *Fe*, *Cd*, *Al*, *Sn*, appear to be real coincidences.

The facts exhibited in the above tables may be summarized as follows:

1. The fluorescent light excited in a crystal of fluor-spar by the light from certain metallic sparks may show in its spectrum many sharp lines and narrow bands. It would seem from the facts at hand that the entire fluorescence spectrum contains a *very great* number of these sharp maxima, a part of which are excited by the light from one spark, another part by another spark, etc., and that it must, in fact, be considered the sum of all the lines that are excited by all the different sparks.

2. Certain of these maxima *appear* to be common to the fluorescence spectra excited by different sparks. The want of accuracy in the measurement of the wave-lengths (2 to 3 tenth-meters) prevents a more definite statement of this important point.

3. Certain of these maxima are evidently peculiar to the light excited by a single metal, as no small error in the determination of the wave-lengths could account for the different distribution of the strong lines in the various spectra produced in the same crystal by different exciting sources.

4. The fluorescence spectrum may vary from crystal to crystal, not only in minor points, but even in the strongest lines.

5. Metals with a strongly marked ultra-violet spectrum excite extended fluorescence spectra.

As is shown directly by the effect of glass and quartz plates, the exciting light for these lines lies between λ 3000, where glass absorbs almost completely, and λ 2000, where quartz begins to show absorption. It is therefore to be expected that metals having strong lines in this region will excite the strongest fluorescence. Magnesium has not a large number of lines in this region, but the lines belonging to it are very strong ones, and the fluorescence spectrum excited by its spark contains as many lines as any of the spectra observed. The metals which excite fluorescence lines of shortest wave-length are, however, iron and aluminium, both of which show strong lines over the region from λ 3000 to λ 2000. Lead, a metal with a comparatively weak ultra-violet spectrum, shows no fluorescence lines whatever.

6. The strong lines in the fluorescence spectra lie between λ 5700 and λ 6400. The whole spectrum excited by a metal like iron extends from λ 4000 to about λ 7000, the part from λ 4000 to λ 4800 being covered by the broad blue band which is excited by light of not much shorter wave-length. The part from λ 4800 to λ 5700 contains many narrow bands and some rather weak sharp lines, and the part from λ 5700 to λ 7000 contains the strong sharp lines.

THE SOURCE OF THE FLUORESCENCE LINES

All the results obtained so far, which have bearing on the mechanism of these phenomena, are of negative character. As this is merely a preliminary note on what it is hoped will prove a more extended

examination of the matter, it may be best to summarize the data at hand.

1. The lines so far measured in the fluorescence spectra of these crystals do not appear to belong to any known substance. The first measurements on the strongest line suggested the spectrum emitted by yttrium compounds when fluorescing in a vacuum tube under the influence of the cathode discharge. The strongest line in the fluorescence spectrum excited by several metals falls on the "citron band," for the cause of which Crookes searched so long, within the limit of accuracy of the measurements. Humphreys[1] has recently shown that yttrium is a very common and perhaps a universal impurity in fluor-spars from many localities. The spectra were therefore carefully compared with Crookes' tables for the lines of the yttrium luminescence, but there were no other even approximate coincidences of strong lines.

2. Although certain fluorescence spectra do vary with the exciting source, shortening or lengthening as the wave-length of the exciting light is increased or diminished, a total change in the distribution of sharp lines over a spectrum, due to a change in the exciting source, is something beyond our ordinary experience of spectrum production.

3. The possibility of optical resonance has been examined, with wholly negative results. Since the lines in the fluorescence spectrum are so sharp, it seemed at first possible that a definite relation between sharp spark lines (in the region from λ 3000 to λ 2000) and the fluorescence lines might be established. Magnesium, for example, has the following sharp strong lines in its fluorescence spectrum: (crystal No. 4) λ 5732, λ 5800, λ 6056, λ 6110, λ 6195, λ 6350. The strongest lines in the *Mg* spectrum between λ 3000 and λ 2000 lie close about λ 2800, with others at λ 2852, λ 2929, λ 2937. No relation between the oscillation-frequencies of spark lines and fluorescence lines has been found. A strong line lying between λ 5730 and λ 5740 is characteristic of the fluorescence spectra produced in the polished crystals by *Fe, Al, Cd, Mg, Sn,* and the ultra-violet spectra of these metals are so very different that it is hardly possible that there should be a simple relation between a strong line in all of these spark spectra and a line of the fluorescence spectra lying within the limits given. Since

[1] *Astrophysical Journal,* **20,** 266, 1904.

certain air lines might be common to all of these metals, and it is possible that they might be the excitant giving common fluorescence lines, especial interest attaches to the spectra produced with inductance in the secondary discharge circuit. The lines of the air spectrum are quite completely cut out by inductance within the region of the comparison photographs, but this can hardly be taken as proof that they are cut out in the ultra-violet as well. Our knowledge of the air lines in the region between λ 3000 and λ 2000 is not very perfect, but the fact that the introduction of inductance in no case alters the relative strength of fluorescence lines may be taken as evidence that the metallic lines are the main exciting source. The possibility of optical resonance is made still more improbable by the fact that the spectrum varies sharply from crystal to crystal with the same exciting source.

4. These new lines of fluorescence may be connected with an impurity in the crystals. All of the crystals which give the sharp lines are colored, and the colored parts are in layers or *strata*. The fluorescence which gives the sharp lines is either confined to these strata or is much stronger in them than in the other parts of the crystal. So far as I know, this stratification of the fluorescing part of fluorite was first noticed by Sir David Brewster.[1] It was also noticed by Stokes[2] as being the source of the red "fluorescence," but, as he did not examine the spectrum, he did not observe the lines produced in the fluorescence of the colored strata.

5. Whatever the source of the fluorescence, it is completely removed by heating the crystal to a temperature above 300° C. Crystals of the purple fluorite when so heated as to lose their power of giving the red fluorescence change their external appearance and become milky and opaque in the strata which were previously colored and which gave most strongly the sharp lines of fluorescence; while the strata which were previously colorless and which did not give the sharp lines retain their transparency. On exposing a crystal which has been so heated to the spark it may again be made to give the *blue* band of fluorescence, but the red fluorescence which yields the sharp lines is not regenerated.

6. The change in the appearance of the fluor-spar on heating

[1] *Pogg. Ann.*, **73**, 531, 1848. [2] *Phil. Trans.*, 1862.

appears to be due to the crushing effect of the expansion of inclusions which were present in the mineral. Examination of thin sections of the mineral shows layers or sheets of inclusions scattered through it, the hollow containing in each case about three-fourths of its volume of liquid, while the remaining space is filled with a bubble of gas. The crystals which had been rendered milky by heating showed on the surface evidence that the included liquid and gas had burst through the inclosing walls and shattered the crystal in all directions. This does not prove that the same effect has been brought about in the deeper parts of the crystal, but the matter seems to deserve further examination.

7. Even though it be admitted that the fluorescence is due to an impurity of a liquid or gaseous nature contained in the inclusions of the mineral, we are brought no nearer to an explanation of the facts connected with the appearance of the *sharp lines* of fluorescence.

The investigation will be continued along two lines: an examination of the spectra under conditions of greater accuracy, and an examination of the substances included in the naturally occurring mineral.

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THE SOLAR ORIGIN OF TERRESTRIAL MAGNETIC DISTURBANCES

By E. WALTER MAUNDER

Early in the year 1904 I communicated to the Royal Astronomical Society two papers on the "Great Magnetic Storms of the period, 1875–1903." The first paper treated in detail the nineteen greatest storms of the period, and gave an account of the state of the Sun's surface at the time of each. It appeared from the comparison that there was a real connection between large sun-spots and great magnetic storms, and a real, but only rough, connection between the size of the spot and the intensity of the storm; the area of the spot-group being by no means an exact index of the degree or intensity of the magnetic disturbance. The second paper suggested a possible explanation of the obvious fact that, though, if we consider a number of instances, there will *on the average* be a distinct approach to a definite relation between the extent of a spot, and the violence of the storm associated with it, yet such a relation does by no means hold good in every individual case. The suggestion made was that it was conceivable that the solar action, giving rise to our magnetic disturbances, was not equally great in all directions, but was most intense in a definite direction, and the analogy of the long rays of the solar corona was appealed to as an example of an action like this in its directive character.

At the time of writing these two papers I was able only to adduce as a proof of my suggestion that we actually found, at the moment of commencement of the nineteen great storms examined, that the most important spot on the Sun was always within a restricted area of the surface. If the influence of the spot were exactly equal over the whole sphere of which it was the center, it is difficult to understand why this relation should have shown itself.

I am now in a position to give a much fuller demonstration that the solar action is directive in character. On the completion of my paper on the nineteen great storms, I tried to extend the same method to all the disturbances represented in the plates of the

Greenwich Magnetic Results from 1882 onwards. These were about 310 in number; when those were excluded in which the greatest amplitude of movement in declination was under 20′, the number left was 276. The detailed comparison of these with the groups of sun-spots on the disk of the Sun at the time of their appearance proved a laborious and unfruitful task. There were a large proportion of cases in which a very plausible argument could be offered in favor of a connection between the disturbance and the particular spot-group. There were yet more instances in which the spot-groups present were too numerous for one or other to be singled out as certainly associated with the disturbance. There were some instances when there were no spots at all on the Sun, or those seen were so insignificant that it seemed absurd to assign any influence to them. In all, the history of over 800 spot-groups was investigated, and though a general correspondence between the spots and the disturbances appeared to be clearly indicated, the individual discordances remained very perplexing.

The next step was an attempt to ascertain if any better correspondence could be found by limiting the inquiry to the spots of either the northern or the southern hemisphere. The faculæ were next tried, then the prominences, and these were tested in several different ways; first the prominences as a whole, then the prominences of either hemisphere, then those within 20°, first of one pole and then of the other, and of the two combined; and still no progress was made. There was a general resemblance, except in the case of the polar prominences, in the very broadest features of the curves, but none reproduced the details of the curves of magnetic disturbances.

At this stage, while looking over the list of the 276 disturbances which I had scheduled, my eye was caught by the four catalogued at the end of 1886, since they succeeded each other at very nearly equal intervals of time, and in the following year a similar set of four presented themselves. The following little table gives the times at which they commenced.

It was clear at once that the mean period indicated by these two sets of disturbances was none other than that of the mean, synodic rotation period of the Sun as derived by Carrington and

Reference Number	Class of Storm	Date of Commencement	Interval	Mean
75	Active	1886 Sept. 9^d 13^h		
76	Active	Oct. 6 17	27^d 4^h	
77	Moderate	Nov. 2 15	26 22	27^d 8^h
78	Active	Nov. 30 13	27 21	
81	Moderate	1887 Aug. 1 11		
82	Moderate	Aug. 28 20	27 9	
83	Very Active	Sept. 25 16	27 20	27 9
84	Moderate	Oct. 22 15	26 23	

others from observation of sun-spots, and I was at once reminded that, in my paper on the nineteen great magnetic storms, sixteen had synchronized with a large or very large group of spots, but the other three had synchronized, not with a large group, but with the *return* of a group which had been large in the preceding rotation. It therefore occurred to me that it would be well worth while to take out the heliographic longitude of the center of the Sun's disk for the times of the commencement of each of the 276 disturbances of my table.

The relations brought out by this computation were most striking. No fewer than thirty-six sets were noticed in which a disturbance in one rotation was followed in the next rotation by another when the same heliographic longitude was again on the center of the Sun's disk. Of these sets twenty-three were pairs, eight were triplets, four extended to a fourth consecutive rotation, and one to a sixth. Besides these, there were sixteen cases in which the interval was not one rotation, but two, so that nearly one-half of all the items in the catalogue were included in one or other of these sequences. It should be further noted that in a large proportion of these cases not only did two disturbances occur at the interval of one or two solar rotations, but no disturbance occurred in the interval. When higher multiples of a rotation period than two were included, it was found that more than three-fourths of the whole number of disturbances catalogued could be grouped in some longer or shorter sequence. In short, the solar rotation period was stamped upon the whole series of disturbances under consideration from beginning to end. It is not too much to say that if the solar disk had

never been diversified by a single marking, if we had never seen a sun-spot, a facula, or a prominence, yet, if the spectroscopic method had made known to us the length of the solar rotation period, the evidence presented by these magnetic disturbances would have proved beyond dispute that they had their origin in the Sun.

This is the first and most important conclusion to which we are led. The suggestion that has been made that sun-spots and magnetic disturbances may be joint effects, both produced by some body or bodies moving in interplanetary space, is finally disproved. The exciting cause of the terrestrial magnetic disturbances is in the Sun, not exterior to it.

Further, the cause, whatever its nature, is not general to the whole surface of the Sun, but is local to certain regions. The solar rotation period could not evidence itself if this were not the case, and if the area whence the magnetic action took its rise were not fairly limited and defined.

The next result is the one which I offered as a suggestion nearly a year ago. This solar "magnetic action,"[1] whatever its nature, can not be radiative like light and heat, equally in all directions. This is, indeed, suggested by the extreme suddenness with which so many storms, and all the greater ones, commence. But so long as it was not recognized that the disturbances manifested the solar rotation period, so long it was possible to conceive of each storm as answering directly to a fresh explosion of energy on the Sun, the time of commencement of the storm representing precisely the time of the actual solar explosion, corrected only for the interval necessary for the effect to travel to us. But it is not possible to conceive that such solar explosions should systematically take place at intervals corresponding to the synodic rotation period.

[1] In speaking of the Sun's "magnetic action," and of "magnetically active" areas upon its surface, I am not in the least desiring to make any physical assumption whatsoever, but am using these expressions merely to indicate that the exciting cause of our magnetic disturbances—whatever its character and mode of operation—arises from the Sun, and from limited areas of it. And in using the words "exciting cause," I am leaving the question entirely open as to whether the Sun actually supplies the energy manifested in these storms, or merely brings about its release. These are not questions within my province; nor, if they were, do they lie within the scope and purpose of my present paper.

It follows that the sudden and definite commencement of the disturbances can only be explained by the collision between the Earth and a well-defined stream; a stream rising and being continually replenished from a limited area on the Sun's surface, and thus appearing to us to travel with the speed of the solar rotation. In other words, we are not struck by the advancing face of a spherical wave, spreading out from the Sun in all directions, but the Earth is overtaken in its orbit by a fairly well-defined stream.

The fourth point is a very interesting one. The various disturbances do not all give precisely the same rotation period; they show a range, though not a very wide one; the mean daily angular motion which they indicate ranging from 14° 32′ to 13° 39′, values agreeing exceedingly closely with those found by Carrington for the equator and for latitude 30°. In other words, the "magnetically active" areas of the Sun are confined to zones the rotation periods for which correspond to those of the zones where sun-spots most congregate. And it has already been shown that in certain instances there can be no reasonable doubt of the direct connection between certain individual spots and certain individual storms.

But it follows at once from what we have already seen as to the directive nature of the Sun's magnetic action, that many spots must fail to exercise any sensible influence upon terrestrial magnetism because in their cases the line of direction does not intercept the ecliptic. The average duration of the disturbances in my catalogue was thirty hours. This would correspond to 16°.5 of heliographic longitude, and would imply that the mean diameter of the stream lines was not greater than 20°, if we suppose them circular in section. We might expect, therefore, that a considerable majority of such stream-lines would completely fail to strike the Earth.

But while we ought thus to have many instances of important spots which produce no effect upon our magnets, there seems clear proof that the magnetic action may continue for long after the spots have ceased to be visible to us. A very striking instance occurred in 1889. As will be seen from the table below, an active disturbance was registered at Greenwich on July 17. Twenty-seven days later another occurred, on August 13. Again twenty-seven days,

and a moderate disturbance took place on September 8, and similarly on October 5, November 1, and November 26; the interval steadily diminishing at each successive return. It is not possible to look at this record, obtained at a time when disturbances were rare, without being convinced that these six were not separate and distinct outbreaks of energy, having no connection with each other, but were indeed returns of one and the same disturbance. If so, they form as a series by far the most important magnetic event of the year, and we naturally inquire whether there was any phase of sun-spot activity to correspond. There was. The largest group of the year synchronized in its second and third returns with the disturbances of July and August, while the spot-group third in importance in the year took its origin shortly before the July disturbance, and synchronized with the one of August at its return. It is, of course, open to question whether we ought to associate the magnetic disturbance with the one group or with the other, or with the joint action of both; but it is striking that the most important and most long continued magnetic disturbance of the year should thus have synchronized with the most important spot-groups. But the spot-groups did not return with the third return of the magnetic disturbance, so that the latter was felt for four successive rotations after the spot-groups had ceased to be visible to us.

Reference Number	Class of Disturbance	Date of Commencement	Longitude of Sun's Center
104	Active	1889 July 17^{d} 4^{h}9	57°.0
105	Moderate	Aug. 13 5	60.0
106	Moderate	Sept. 8 23	66.5
108	Moderate	Oct. 5 17	73.4
111	Active	Nov. 1 6	83.4
112	Active	Nov. 26 15	108.8

Perhaps the most striking instance of the return of a magnetic storm with the return of a spot was furnished by the great group of February 1892, much the largest group of the last thirty years. The mean longitude of this group, during its apparition in February, when it attained its greatest area, was 255°.7. The great magnetic storm began when it was 17°.5 west of the central merid-

ian; that is to say, when the longitude of the Sun's center was 238°.2. When the group returned in March, it had very greatly diminished in size; its mean longitude in this apparition was 250°.3, and the second great magnetic storm commenced when it was 17°.1 west of the center of the Sun, that is to say, when the longitude of the Sun's center was 233°.2. The correspondence therefore between spot and storm was exact to four-tenths of a degree of solar longitude, or less than three-quarters of an hour of time; a striking correspondence when it is remembered that the group was some 24° of solar longitude in length at its greatest development, and took forty-four hours to cross the central meridian.

It is perhaps worthy of note that in the following year, 1893, magnetic disturbances occurred in five rotations out of the thirteen, when this same meridian (235°±) came to the center of the disk. The most striking of these synchronized with a revival of activity in the same region of the Sun; two groups of sun-spots forming in July 1893 within the area covered by the great group of February 1892. It must not be supposed that whenever a spot broke out in this region a marked magnetic disturbance always accompanied it, or that when a disturbance was experienced at this particular meridian spots were always seen to correspond. But the intermittency in the activity of sun-spots and of disturbances have the same general character, though that activity is by no means always manifested at the same time in the two phenomena. One other relation calls for notice. There seems to be a distinct tendency for magnetic disturbances to occur at the end of thirteen or fourteen rotations; that is to say, as nearly as possible at the end of a solar year. The most striking case of this is afforded by a comparison of the years 1899 and 1900. My catalogue contains only three disturbances in the latter year. As will be seen from the following table, in each case a disturbance, almost exactly at the same solar longitude, occurred just thirteen rotations earlier.

There is no difficulty in conceiving the manner in which the solar action giving rise to our magnetic storms may be conveyed to us. The corona has been at some trouble, these many years past, to visualize for us an action which is at least analogous to that in question. For myself, the first hint of the idea came with

Date of Commencement	Longitude of Sun's Center	Date of Commencement	Longitude of Sun's Center
1899 Jan. 28d 19h	172°.2	1900 Jan. 19d 18h	153°.7
Mar. 23 15	183.0	Mar. 13 2	184.5
May 15 13	204.1	May 5 3	204.4

my first sight of a total solar eclipse in 1886. From that time I have never had the slightest doubt but that at least the polar plumes are strictly analogous to the tails of comets; that is, that they consist of very minute particles driven away from the Sun by some repulsive force; whether that might be electrical, or due to the pressure of radiation or to some other cause, is not at this point a matter of importance. In 1898 I was led, from the examination of the photographs taken by the expedition of the British Astronomical Association at Talni, India, to recognize that the synclinal structures were also formed by a repulsive action; Mr. Thwaites's negatives showed the formations at the base of those structures; Mrs. Maunder's showed those structures drawn out into long rod-like rays at the apex. True, none of the actual rays then photographed could have encountered our Earth, their inclination to the ecliptic being too considerable; but, having seen so clearly upon the photographs the intimate building up of those rays, it became impossible to contend that similar rays might not from time to time be formed which would actually overtake us.

It will be seen that the rélation, brought out by my inquiry, is of the very simplest; most easy to detect and to demonstrate; it is simply that our magnetic disturbances show a very strongly marked tendency to recur with the return of certain definite meridians of the Sun to the center of the disk. But the consequences are revolutionary. First of all, it is clear that our magnetic disturbances do have their origin in the Sun, and not in any interplanetary corpuscles, or in the varying electrical conductivity of extra-solar space if this be conceived as varying independently of solar action. Next, the solar magnetic action, giving rise to these disturbances, is not from the whole surface, but from certain restricted areas. These areas are clearly connected with sun-spots, since the disturbances occur at intervals correspond-

ing to the rotation period of the Sun, as determined by sun-spots, and to the special rotation periods of the chief sun-spot zones. Third, these areas can be magnetically active both before a spot has formed and after it has disappeared. Their activity is, in the general way, most easily and continuously manifested to us by the presence of sun-spots, and by the changes which they undergo; but sun-spot formation can be considered only one phase of their activity, and possibly not the most important and significant one. Lastly, the action from these special areas is not radiative, like light and heat, but directive, along narrow, well-defined streams.

OBJECTIONS

Several difficulties have occurred to me in the course of my inquiry. The first is connected with the very small area which the Earth presents to the Sun. Unless the stream-lines from a spot region are of very considerable cross-section, it would seem to be most unlikely that any one of them should strike the Earth; whereas a large majority of very great sun-spots are associated with considerable disturbances; and, moreover, these sun-spots are often in very high latitudes, and may pass the center of the disk at a distance of 30° or even 35° from it. Again, some of the sequences, in which the disturbances appear to run, extend more or less intermittently through the entire year, or at any rate from March to September, or September to March; that is to say, it appears to be a matter of indifference to them as to whether the latitude of the center of the Sun's disk has its greatest southern or its greatest northern value. A further difficulty lies in the fact that, though on the average a magnetic disturbance will commence when the sun-spot with which it would seem to be associated has passed the central meridian by several hours, yet occasionally it begins before the spot has reached the center.

These and similar difficulties are no doubt serious, and at present I am not in a position to do more than suggest partial and possible explanations. In the first place, if we may be guided by the coronal forms, it is clear that the solar stream-lines need not be truly radial. Of the four great rod-like rays photographed during the eclipse of 1898, one appeared almost tangential to the Sun's limb, one

was inclined, and two appeared to be radial. Whether they were really radial or not we cannot say, for, while a streamer which is truly radial, must appear to be so from whatever position it is viewed, the converse does not hold good. We have therefore no *a priori* right to assert that the stream-line from a spot in high latitude, or from one that has not yet reached the central meridian, might not encounter the Earth.

The difficulty of the frequency of our magnetic storms may be met in two ways. It is possible that the solar streams tend to diverge after a certain distance from the Sun, so that they cover an area of many degrees by the time they have reached the Earth's orbit. The other explanation, which is more in accordance with what the forms of the corona seem to indicate, is that, from one cause or another—possibly as an indirect effect of the solar rotation—there is a tendency in all these stream-lines to be directed toward the Sun's equator. It seems improbable that the stream-lines can have any very great cross-section at the Earth's orbit. The mean duration of the disturbances in my catalogue—thirty hours—implies, as stated above, a diameter of 20°. But this is to suppose, what is most unlikely, that the disturbance ceases the instant the stream-line has entirely passed the Earth; it is far more probable that the impact of the stream would set up an excitement, which would by no means soon come to rest. Indeed, I think that the examination of the trace of a typical disturbance suggests that this is exactly what does occur; that there is a sudden dislocation of the magnetic equilibrium, continuing only for a relatively short space of time, and after the exciting cause has passed, the magnets slowly recover their usual stability. In this case we might regard the average diameter of a stream-line as probably not more than two or three degrees.

A third suggestion has been made which would effectually meet this difficulty, but it seems to me that we have at present no sufficient evidence to enable us to discuss it. It is that the Earth, and the planets generally, may exercise an attractive influence on the solar stream-lines.

The difficulty of the apparent indifference shown by certain sets of disturbances to the change in the latitude of the Sun's center,

is not so serious as it might appear. It is only some sequences which show it, and it probably is apparent only, being due to the very marked sympathy between the two hemispheres of the Sun. The formation of a large spot in a given longitude in one hemisphere is not infrequently answered by the formation of a second in the same longitude, but in the other hemisphere, so that we should rather expect some instances of this character than be surprised when they occur. While the tendency of disturbances to recur when the same meridian is on the center of the disk at the end of a year certainly looks as if some at least of the stream-lines only encountered the Earth at a precise node.

INQUIRIES SUGGESTED

The first point brought out by this new view of the solar relation to our magnetic disturbances is the absolute necessity of comparing together the actual traces obtained at magnetic observatories, widely separated from each other, and, in particular, observatories placed on different sides of the equator. The first is necessary that we may be able to distinguish between those disturbances which are truly cosmical and those which are telluric. When the latter are eliminated, a comparison of the records from observatories with opposed seasons should enable us to decide whether the annual inequality, observable in the number and intensity of disturbances, is due solely to the march of the seasons, or to the position of the Sun's equator; or, as the disturbances registered at Greenwich seem to me to suggest, to a combination of both causes.

But apart from the necessity of distinguishing between the disturbances of cosmical and those of telluric origin, the comparison of the intimate details of the traces from observatories differing widely in longitude, ought to be undertaken. A solar streamline, overtaking the Earth, will necessarily strike it first on the sunset arc, and will pass across the hemisphere in daylight; the advancing edge of the stream taking just half a minute to pass from the sunset arc to the arc of sunrise. We might expect that there would be some perceptible difference between the behavior of the magnets in the hemisphere in light, upon which the impact

of the stream is direct and immediate, and that of those in the hemisphere in darkness, which would receive their disturbing currents, as it were, at second hand, since they would be protected from the actual impact of the solar particles; or, in other words, that there would be some difference in the character of a disturbance, depending on the local time at the observing station.

The suggestion made above, that the solar stream-lines must strike the Earth first on the sunset side, raises a point to be determined by the observation of auroræ. These are necessarily seen only in the hemisphere in darkness, but it is conceivable that the general character and progress of the phenomenon, as seen from any given station, may depend upon the position of the Earth at the moment when the stream from the Sun first strikes the Earth; that is to say, when the magnetic disturbance has its first sharp beginning.

Seeing that our magnetic disturbances are due to a solar action, taking place along definite lines, some of which strike the Earth, and some of which do not, the question at once arises: What would happen if one of these streams should just graze the Earth? Would the effect be the same as if it struck it centrally, or would there be a marked difference between the character of the disturbance as registered at a station within the region actually struck by the stream, and as registered at one at a very great distance from that region? More important still: What will happen if one of these streams should fail to strike the Earth at all, but should pass near it either to the north or the south? Will it produce no effect at all, or is it possible that it may give rise to an induced current? In the latter case may it be that the characteristic sharp twitch, instantaneous over the whole Earth, so typical of the more violent storms, denotes that a stream-line has come into actual collision with the Earth, while the more gradual, sluggish, and less-defined disturbances represent the effect of the passage of one of these streams either above or below the Earth without actually encountering it? Clearly, if this conception be admissible, it will do something to remove some of the difficulties suggested earlier. The number of streams actually striking the Earth may be very much smaller than the number of observed disturbances, seeing that many of them may

be due to such near approaches. It renders less serious the difficulty arising from the altered latitude of the center of the disk at the successive returns of a disturbance. We may, in fact, not regard the Earth merely as a globe 8".8 in radius, for it may be sensitive to streams passing within an undefined distance from it, and thus the effective "catchment area" may be several times as great as the apparent disk of the Earth viewed from the Sun.

If the magnetic disturbances are due to these streams of repelled particles, what is the cause of the cyclical inequality in the diurnal range? Nothing is clearer than that it varies in general sympathy with the spot-activity of the Sun. We should naturally, therefore, expect to find it due to a cause of the same general character as that giving rise to the disturbances. Yet this cyclical inequality is of about the same magnitude as, and is similar in character to, the annual inequality; the diurnal range being greatest in the years of the eleven-year cycle when there are most sun-spots, just as it is also greatest in the months of summer. This would lead us to expect that the annual inequality may not differ in essence from the cyclical, and that both may conceivably be due to the influence of great numbers of very attenuated, feeble, sparse streams, thrown off by the Sun at all times and in many directions, though most abundantly at sun-spot maximum—streams which might individually bear about the same relation to the great rod-like rays seen in the Indian eclipse, or to the stream causing the storm of November 1882, as the minute pores of the Sun's surface, which alternate everywhere with the photospheric granules, do to some gigantic spot, fifty thousand miles in diameter. The varying presentation of different regions of the Earth toward the Sun at different times of the year, involving a greater or less exposure to this influence, would be the cause of the annual inequality, while the varying presentation during the Earth's daily rotation on its axis would account for the diurnal range itself.

Are the particles reaching us from the Sun all of the same size, or, what comes to the same thing, do they all travel with the same rapidity? It is scarcely conceivable that such should be the case. But if not, then it would follow that a great storm would tend to reach us in a succession of disturbances. If it arose from a region

marked by a great spot, that spot would appear to occupy different positions on the disk at the commencement of the different sections of the disturbance; and if it were possible to look down upon the solar system from above, and to detect these streams, instead of a single ray coming from one of these areas of disturbance, we should see a fan-shaped cluster of them, just as we have tails of different types diverging from the heads of comets. It is possible that the explanation of the successive arches, arch above arch, which have been seen in the synclinal structures of the corona, are actual evidence of such different rates of motion, and the careful measurement of the intervals between successive outbursts of activity in the course of a given storm might furnish us with the means of determining the relative speeds with which the particles repelled from the Sun have traveled to us. One consequence, however, should follow: the plane of the fan should lie either in the plane of the Sun's equator, or be parallel to it, so that at the beginning of June and the beginning of December, when the Sun's equator is most inclined to the ecliptic, there would be the greatest tendency for some members of the fan to miss the Earth. At the beginning of March and the beginning of September there would be the greatest tendency, if one member of the fan struck the Earth, for all to do so. In short, disturbances would show a strong tendency, on the average, to be most prolonged just before our equinoxes, and to be shortest just before our solstices. That there are more disturbed days at our equinoxes than at our solstices is a relation to which Mr. W. Ellis has already drawn attention, and perhaps this is where we may find its explanation.

APPENDIX

As an example of the kind of evidence offered by the magnetic disturbances recorded at Greenwich of the influence of the synodic rotation period of the Sun upon them, I give here the sets of disturbances drawn from those which I have catalogued for the years 1895 to 1898. The letters G, V, A, M, in the second column signify "great," "very active," "active," and "moderate;" a "great" disturbance being one in which the maximum amplitude of movement in declination exceeds 60′; a "very active," in which it exceeds 40′; an "active," 30′; and a "moderate," 20.′ The same numeration of the rotations, the same prime meridian and the same sidereal rotation period (25.38 days) have been adopted as in the "Heliographic Results" given in the annual volumes of the *Greenwich Observations.*

Reference Number of Disturbance	Class	Time of Commencement	Number of Rotation	Longitude of Sun's Center	Latitude of Sun's Center
196	M	1895 Feb. 8^d 13^h	553	234°0	−6°6
197	A	9 11	...	222.0	−6.7
199	A	Mar. 8 14	554	224.7	−7.3
203	M	May 29 3	557	228.0	−0.9
205	A	Oct. 12 16	562	222.8	+6.0
206	A	Nov. 9 13	563	215.2	+3.3
207	A	1896 Jan. 3 0	565	217.7	−3.5
208	M	31 12	566	202.4	−6.1
210	V	Feb. 28 9	567	195.4	−7.2
212	M	Mar. 26 13	568	197.2	−6.7
214	A	1896 May 17 12	570	230.9	−2.1
215	M	July 11 15	572	221.3	+4.2
218	M	Aug. 6 16	573	236.8	+6.3
234	V	1897 Dec. 11 3	591	230.8	−0.7
245	G	1898 Sept. 9 14	601	233.6	+7.2
198	M	1895 Feb. 15 14	553	141.3	−6.9
201	A	Apr. 11 6	555	140.7	−5.8
209	A	1896 Feb. 4 12	566	149.7	−6.4
213	V	1896 May 2 13	569	68.6	−3.8
216	M	July 23 18	572	60.9	+5.3
217	A	1896 Aug. 1 12	573	305.2	+6.0
219	M	29 17	574	292.6	+7.2
222	A	1896 Oct. 11 13	575	86.9	+6.0
223	A	Nov. 7 12	576	91.5	+3.4
224	V	Dec. 3 17	577	105.9	+0.3
225	A	1897 Jan. 2 12	578	73.5	−3.5
226	M	Feb. 25 15	580	80.8	−7.2
229	A	Apr. 20 12	582	90.3	−5.0
230	M	1897 Apr. 23 13	582	50.1	−4.7
231	M	May 20 21	583	48.7	−1.8
232	M	1897 Sept. 4 13	587	77.7	+7.3
233	M	Oct. 1 20	588	77.5	+6.6
235	V	1897 Dec. 20 13	591	106.6	−1.9
236	M	1898 Jan. 16 16	592	109.4	−4.9
238	M	Feb. 12 14	593	115.0	−6.8
240	M	Mar. 11 14	594	119.8	−7.2
239	M	1898 Feb. 14 13	593	89.2	−6.9
241	G	Mar. 15 1	594	73.9	−7.1
242	M	Apr. 12 16	595	56.2	−5.7
246	A	1898 Oct. 25 12	603	347.8	+4.8
247	M	Nov. 21 12	604	351.8	+1.9

Out of the fifty-two disturbances catalogued in these four years no fewer than forty-one fall into one or other of the above sets.

86 TYRWHITT ROAD,
St. John's, London, S. E.,
December 12, 1904.

THE EFFECT OF CAPACITY AND SELF-INDUCTION UPON WAVE-LENGTH IN THE SPARK SPECTRUM

By GEORGE W. MIDDLEKAUFF

INTRODUCTION

In line of sight work the comparison spectrum is usually that of the spark. For this reason it is a matter of importance to know whether or not the wave-lengths of the spark spectrum remain constant under varying external conditions of capacity, self-induction, etc. Although considerable work has been done on this subject during the last few years, there is still such a diversity of opinion among the observers that the question cannot be considered as settled. In view of this fact, Professor Ames suggested to the writer that an investigation along this line, with the facilities offered in the physical laboratory of the Johns Hopkins University, would be of interest. Accordingly this work was carried out in the spring of 1903.

The iron spectrum was selected as the subject of study, and the work was undertaken with two principal objects in view, namely, (1) to produce the spectrum under the greatest possible variations in capacity and self-induction, and find the effect, if any, upon the wave-lengths of the lines; and (2), to make accurate measurement of the lines in the spark spectrum throughout the region utilized by line of sight observers.

EFFECT OF CAPACITY AND SELF-INDUCTION UPON WAVE-LENGTH

Historical review.—Exner and Haschek,[1] in the course of their measurements of the ultra-violet spark spectra of the elements, observed that in many instances the wave-lengths of lines in the spark spectrum differed considerably from the wave-lengths of the corresponding lines in the arc, the difference being often as much as 0.3 of an Ångström unit. This difference they considered as being due to pressure in the spark, and with data taken from the tables of Humphreys and Mohler[2] they estimated the pressure in the spark to be from 24 to 30 atmospheres.

[1] *Sitzungsberichte der Kais. Akad. der Wiss. in Wien*, 1897 *et seq.*

[2] *Astrophysical Journal*, 1896 and 1897.

Then Haschek and Mache[1] undertook to measure this assumed pressure by placing the spark in an air-tight vessel filled with air and furnished with a manometer. On discharging the spark they observed a sudden rise in the manometer, and interpreted it as a manifestation of a pressure in the spark. However, the value they thus obtained was far greater than the value of the pressure calculated from the difference in wave-length between arc and spark lines. They further found[2] that as they increased the amount of capacity across the discharge circuit the effect was increased to a maximum and then decreased. The potential used in the experiment was furnished by a powerful transformer.

Then Mohler[3] with the use of an induction coil and a Rowland grating, found that with an increase of capacity there was an increase in the shift of the lines which seemed to approach a maximum value; but the calculated pressures obtained were by no means as great as those found by Haschek and Mache.

In responding to Mohler's results, Haschek and Mache[4] claim that the former's work is but a confirmation of their own, the difference in value being due to the difference in energy produced by a transformer and an induction coil, respectively.

Contrary to the results of the above observers, Eder and Valenta,[5] on investigating some of the lines in which Exner and Haschek believed they had found an increase in wave-length due to capacity, obtained photographs which indicated no change whatever.

Haschek,[6] from later experiments, concluded that in many instances lines which in the spark were increased in wave-length by equal amounts were increased by unequal amounts in the arc under pressure, and conversely; also that even in the spectrum of the same element the wave-lengths of the lines were increased by unequal amounts, indicating that other influences besides pressure were making themselves felt; and hence the pressure in the spark, if it be a reality, cannot be determined directly from any one line of the spectrum. From the same experiments he concluded, further, that there were increases

[1] *Sitzungsberichte der Kais. Akad. der Wiss. in Wien*, 1898.

[2] *Astrophysical Journal*, **9**, 351, 1899.

[3] *Ibid.*, **10**, 204, 1899.

[4] *Ibid.*, **12**, 50, 1900.

[5] Kayser, *Handbuch der Spectroscopie*, **2**, 308.

[6] *Astrophysical Journal*, **14**, 187, 1901.

in wave-length produced by increasing the amount of luminous vapor in the spark. This, however, is contrary to the results of Humphreys and Mohler,[1] who found that in the case of the arc pressure only and not vapor caused an increase in wave-length of the lines. It is also contrary to the opinion of Kayser,[2] who states that in all his investigations of arc spectra he observed no change in wave-length due to the amount of vapor present in the arc.

Then Kent[3] undertook to verify the results of Haschek, and concluded that in the case of titanium there was a shift due to capacity, but that it did not amount to more than 0.04 of an Ångström unit for lines in which Haschek observed as much as 0.13 of a unit. Later this same observer made an investigation[4] of the possible effect of various circuit conditions upon the wave-length of the lines in the spark spectrum, using titanium as the subject of study. He concluded that there were changes in wave-length produced by changes in capacity, self-induction, resistance in field circuit of alternator, impedance in the primary circuit of the transformer, length of spark-gap, etc. In short, he concluded that almost any variation in the condition of the electric circuit would produce a corresponding change in the wave-length of the lines of the spark spectrum.

THE PRESENT INVESTIGATION

Apparatus.—The grating employed in this investigation was a twenty-one and a half foot concave Rowland grating with 20,000 lines to the inch, mounted as described by Professor Ames in the *Philosophical Magazine*, p. 369, 1889, and being in all respects the same instrument used by Humphreys and Mohler in their work on pressure shift. For the purpose of comparison of spectra, the camera carries a shutter consisting of a long brass plate, capable of revolution around a horizontal axis, and having throughout its length a longitudinal opening of a width equal to the thickness of the brass plate; so that when the shutter is in a vertical position the photographic plate is exposed along a strip extending over the whole length of the plate and over a width equal to the thickness of the shutter. When

[1] *Ibid.*, **3**, 114–137, 1896.

[2] *Handbuch der Spectroscopie*, **2**, 297, 308–310.

[3] *Astrophysical Journal*, **14**, 201, 1901.

[4] *Ibid.*, **17**, 286, 1903.

the shutter is turned horizontal, the central strip is covered, and the remainder of the photographic plate above and below the strip is exposed. To avoid any jarring of the camera when the shutter was turned between comparison spectra, the shutter was detached from the camera and supported by two iron clamp-stands which stood upon the floor. As the beam on which the camera rested was supported by the walls of the building, there could be no possible jarring due to the motion of the shutter. In addition to the shutter already described, there were three other shutters placed a few inches in front of it. These were each about seven inches long, were placed end to end, and were capable of turning about the same horizontal axis. They were also supported by the clamp-stands mentioned above. When all three were in a vertical position, no light from the grating could fall upon the photographic plate. When the left-hand one, say, was in a horizontal position and the other two vertical, only the left end of the plate could be exposed, no matter how the first shutter with the opening in it was turned. With the middle one horizontal and the end ones vertical, only the middle portion of the plate could be exposed, etc. These four shutters were operated by means of cords leading to a point near the slit. This was done so that the operator need not move from a fixed position during the exposure of the plate, and he might thus avoid possible shaking of the apparatus due to his walking over the floor.

The spark was produced by a 50-volt alternating current of about 25 amperes with a frequency of 133 cycles per second. After being stepped up to about 600 volts, it was passed through a transformer, which raised the potential sufficiently to produce a spark about two centimeters in length.

The spark terminals were cylindrical rods of Norway iron, 8 mm in diameter and turned down at the spark ends to the shape of a blunt lead-pencil point. These points would wear down slightly during a long exposure, but care was taken always to turn them to the proper shape before each and every exposure. They were given this shape so that the spark would keep on the slit all the time of exposure, and not jump about as it would with terminals more blunt and flat.

An auxiliary spark-gap was tried in series with the main spark-gap, but as there was no appreciable improvement in the spark, so

far as could be noticed by the eye or ear, no photographs were taken with it in.

The arc for comparison spectra, in the case of plates to be measured for standards, was produced by a 110-volt direct current of about 10 amperes. The arc terminals were also of Norway iron rods, these being about a centimeter in diameter.

Both the arc and spark lamps were placed (the arc nearer the slit) upon a platform which was movable upon two knife-edge ways in a direction parallel to a line drawn through the centers of the slit and grating respectively. Stops were placed before and behind the platform at such a distance from it that when it was against one of them the arc was in focus, and when it was against the other the spark was in focus; that is, in both cases the source of light was in precisely the same place at a distance of 130 cm from the slit.

The light was focused upon the slit by means of a quartz condensing lens, which was never disturbed during the exposure on any one given plate, so that all exposures would have the grating as nearly as possible equally illuminated. The spark-gap was usually 8 or 9 mm in length and its image upon the slit about 6 mm. Care was taken always to have the image as symmetrical as possible with respect to the slit; but in many instances, however, especially with the self-induction in circuit, the image during exposure passed through many unsymmetrical forms.

Immediately back of the slit was a card screen supported by a beam perfectly independent of the mounting of the grating. This screen was movable about a horizontal axis and could be raised or lowered from the slit without disturbing the rest of the apparatus.

The capacity consisted of plates of copper foil and glass, all surrounded with paraffin, and could be varied from 0.0085 to 0.0739 of a microfarad. The self-induction apparatus consisted of three coils of No. 10 copper wire, and the amount of induction could be varied from about 0.00007 to 0.0012 of a henry. The whole of the induction, or any part of it, could be thrown in or out of circuit without stopping the spark.

The plates used were Seed's No. 27 and Cramer's Isochromatic, all cut to $1\frac{1}{4}$ by 19 inches, and were carefully developed with a hydrochinon solution according to Jewell's formula.[1]

[1] *Astrophysical Journal*, 11, 240, 1900.

The true photographic focus of the grating was carefully determined by empirical settings, and the time of exposure varied from 15 to 60 seconds for the arc, 3 to 10 minutes for the spark without self-induction, and from 10 to 60 minutes for the spark with self-induction.

All measurements on plates were made with the dividing engine which is fully described by Humphreys in the *Astrophysical Journal*, **6**, 180, 1897, and also by the method described at the same place.

Method of exposure.—So far as known to the writer, all observers on the shift of spark lines have used an arc spectrum for comparison, and have photographed the two spectra on the same plate. This, of course, necessitated the change of light-source and the possibility of error due to the two sources not being in turn at exactly the same point.

To avoid error from this source, the method adopted in the present investigation was to compare the spectra of the *same* spark under any two given external conditions. Since the capacity and self-induction could be thrown in or out by means of switches, the spark was kept going from the beginning to the end of all exposing on any one plate.

The general plan of exposure will be understood from the following example. Suppose the plate is to be taken to study the effect of self-induction. The spark is first tried with and without self-induction in series to find out whether the image falls on the slit and whether the grating is properly illuminated in both instances. Having made any necessary adjustments, the self-induction is thrown out, and the shutter at the slit is turned down to keep the light from the spark off the grating until the photographic plate is adjusted in the camera. With the camera shutter, *A*, vertical and the other three, *B*, *C*, *D*, horizontal, the plate is ready to be exposed over the strip *ab* along its whole length. The shutter at the slit is now raised and the exposure begun. When the proper time for the exposure has expired, the shutters *C*, *D*, and *A* are turned in the order named, so that now the portions 1 and 2 of only the end section of the plate are exposed. When these are exposed as long as *ab* was, *B* is turned up and the self-induction is thrown in circuit; after which the shutter, *C*, is let down to expose the portions 3 and 4. This exposure being finished, *C* is turned up, the self-induction is thrown out, and

the shutter *D* is let down, thus exposing 5 and 6 under external conditions identical with those under which the middle strip *ab* and the sections 1 and 2 were exposed. Hence if no mechanical shift occurred during the exposure, the lines in 1 and 2, and 5 and 6, will be exactly in line with the corresponding lines in the section *ab*. Or, if the apparatus was disturbed during the exposure of 3 and 4, causing the lines in them to be shifted, the lines in 5 and 6 will be shifted an equal amount. If, however, the lines in both the end sections are perfectly in line with those in the strip *ab*, and those in 3 and 4 are not, we can be sure that the difference is a *true* shift.

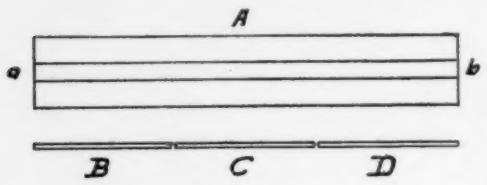

Fig 1
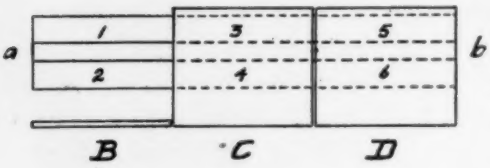
Fig. 2

Results.—A large number of plates were taken by the method described above, to test for a shift due to self-induction; and it was invariably found that, so far as the accuracy of measurement permitted, no shift ever occurred when the self-induction was thrown in circuit. The self-induction spectrum was always photographed in *C* the middle section, sometimes outside, at other times inside. The order in which the sections were taken was also varied, but in every case the self-induction spectrum was checked before and after its exposure by a spectrum without self-induction and with the capacity the same.

A number of plates were also taken to compare the spectra of the spark with large and small capacities, the variations being made from 0.0085 to 0.0739 of a microfarad. In these the lines were, of course, quite broad; but within the limits of accuracy of the measurements no sensible change in the position of the lines was observed.

In the measurement of the lines on all these plates, great care was taken to make the settings, not on the middle of such lines as were unsymmetrically broadened, but on the most intense portion, or center of gravity. For the purpose of measurement, the most

useful plates were those of short exposure, because on them the broadened and diffused portions of the lines were the least extended.

A number of plates were also taken of the titanium spectrum in the region observed by Kent. The same variations were made in capacity and self-induction as in the case of iron; but, as before, the shift, if any, was too small to be detected.

Hence the conclusion to be drawn from the present research is that in the case of a spark discharge in air at atmospheric pressure, there is no change produced in wave-length by variations of capacity or self-induction within the range specified.

A further conclusion, which has been reached from a careful examination by the microscope of all the many photographs taken with the arc and spark spectra on the same plate, is that so far as can thus be determined, there is no appreciable difference in wave-length between the same lines as observed in the arc and spark spectra.

Since the completion of this investigation, Eder and Valenta[1] have published the results of an attempt to verify the conclusions of Haschek in regard to the effect upon wave-length of the amount of luminous vapor present in the spark. In their experimental arrangements they followed strictly those conditions under which Haschek believed he found shifts. They employed powerful induction coils with a sparking distance of from 12 to 25 cm and correspondingly large condensers across the discharge circuit. Even with such extreme conditions, they came to the following conclusions: "(1) no displacements of measurable magnitude occur in the spark spectrum of zinc as compared with the lines of the arc; (2) the quantity of the element present, or the partial pressure of its vapor, produces no displacement of the lines of the spark spectrum."

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[1] *Sitzungsberichte der k. Akad. der Wiss. in Wien*, **112**, IIa. Communicated October 22, 1903. Translation, *Astrophysical Journal*, **19**, 251, 1904.

A STUDY OF THE CONDITIONS FOR SOLAR RESEARCH AT MOUNT WILSON, CALIFORNIA[1]

By GEORGE E. HALE

In 1902, Dr. S. P. Langley addressed a communication to the Carnegie Institution recommending the establishment of an observatory at a very high altitude for the special purpose of measuring the solar radiation. In this communication Dr. Langley offered reasons for his belief that the solar radiation may undergo changes of intensity corresponding with those great changes of solar activity which are so strikingly illustrated in the sun-spot cycle. This communication was referred to an advisory committee appointed by the Carnegie Institution to report on various astronomical projects which had been submitted. The committee consisted of Professor E. C. Pickering, chairman; Professor Lewis Boss, Dr. S. P. Langley, Professor Simon Newcomb, and the writer. In its report to the Carnegie Institution, the committee expressed its approval of Dr. Langley's proposal and recommended, in case the Institution felt inclined to pursue the matter further, that a special committee be appointed to make a detailed report on the requirements of a complete solar observatory. It was also recommended that a project for an observatory in the southern hemisphere be investigated and reported upon by the same committee.

As a result of this recommendation, a committee, consisting of Professor Lewis Boss, chairman; Professor W. W. Campbell, and the writer, was appointed in December 1902 to report upon the proposed southern and solar observatories. The report of this committee may be found in *Year Book* No. 2 of the Carnegie Institution. This report also includes a detailed account by Professor W. J. Hussey of his telescopic tests of atmospheric conditions at sites in southern California and Arizona, where he had been sent by the committee. Professor Hussey strongly recommended, as the result of his tests, that Mount Wilson, near Pasadena, Cal., be chosen as the site of the proposed solar observatory, in case the Carnegie Institution decided to establish it.

[1] *Year Book No. 3 of the Carnegie Institution of Washington.*

My first visit to Mount Wilson was made in company with Professor Campbell in June 1903. Professor Hussey had practically completed his tests and desired that we should see for ourselves the conditions he had found. Previous observations of the Sun at Pike's Peak, Mount Etna, and Mount Hamilton had in no wise prepared me for my experience on Mount Wilson. On certain occasions, it is true, I had seen the solar image sharply defined on Mount Etna in the very early morning hours. On Mount Hamilton, also, the solar image is sometimes good; but the testimony of those who have observed the Sun there was decidedly unfavorable. It was therefore with intense satisfaction that on each of the four days of my stay on Mount Wilson I found the definition of the solar image almost perfect, to be rated at from 8 to 9 on a scale of 10.

This visit was necessarily a hurried one, and it was evident that before Mount Wilson could be determined upon as the best available site for an observatory, observations extending over a long period of time would be necessary. As circumstances required that my family should spend the winter of 1903–4 in southern California, I decided to take this opportunity to make a more complete test of atmospheric conditions on Mount Wilson. Before arrangements had been made for living upon the mountain, I made frequent trips from Pasadena to Mount Wilson during the months of December, January, and February, observing the Sun on each occasion with a telescope of 3¼ inches aperture, and noting the prevailing weather conditions. The extraordinary absence of wind, which had seemed so characteristic a feature of the mountain during Professor Hussey's visit, could not be said to continue throughout the winter months. High gales sometimes occur at this season, and the average wind velocity is greater than during the summer. Nevertheless, the wind during the day was usually very light, and on many occasions the quiet days of the previous June seemed to be almost exactly duplicated, except that the temperature was lower. For weeks together not a cloud would be seen in the sky, and the summer serenity was in some measure retained until well into January. Later it was broken by storms, but these practically ended with April.

As the solar definition proved to be surprisingly good for this season of the year, I was soon convinced that Mount Wilson offered

exceptional opportunities for both solar and stellar work and that a systematic test of conditions should be inaugurated at the earliest possible moment. Accordingly, I commenced on March 1 to render habitable an old log cabin on the mountain that had been in a state of partial ruin for many years. The cabin, known locally as the "Casino," became our headquarters, where we have lived throughout our work on Mount Wilson. Tests of the solar definition were made as often as possible with the telescope already mentioned, and on April 15 several meteorological instruments provided by the Carnegie Institution were installed. Since that time, with only such interruptions as have been made necessary by the enforced absence of the observers, the instruments have been read at stated hours by Mr. Ferdinand Ellerman or Mr. W. S. Adams, who have also made regular tests of the seeing with the telescope mentioned above.

Through important financial assistance rendered by Mr. Arthur Orr, of Evanston, Ill., and Mr. John D. Hooker, of Los Angeles, and the exceptional facilities kindly granted by the Atchison, Topeka & Santa Fé Railway Co., through President Ripley, it became possible to bring from the Yerkes Observatory the small cœlostat which had previously been sent to the eclipses of 1900 (North Carolina) and 1901 (Sumatra). It had been my purpose to bring out the Snow telescope, but lack of sufficient funds prevented me from doing so. The smaller cœlostat was accordingly erected on the mountain, where it yielded excellent photographs of the Sun, amply sufficient to give objective evidence of the high quality of the observational conditions.

During my first visit to Mount Wilson the only unfavorable feature was the presence of fine dust in the air, which was conspicuous not only in the valley below, but also seemed to extend to a considerable altitude above the mountain. This was by no means sufficient to affect greatly the transparency of the sky, except very near the horizon. Nevertheless, the Milky Way did not stand out with the degree of contrast which one expects to see in a very transparent atmosphere. On my return trip to Chicago through the San Gabriel Valley the dust seemed so conspicuous that I feared it might prove an important objection to Mount Wilson as a site for an observatory. In most classes of solar observation dust does not play a very important part, and the great steadiness of the image would far outweigh any objec-

tions which might result from this cause. But in other classes of work which were contemplated for the proposed observatory, this dust, if persistent, would inevitably prove a serious obstacle. For example, in determinations of the value of the solar constant and in the photography of faint nebulæ, the absorption and scattering of light produced by dust in the atmosphere may interfere greatly with the work. It accordingly seemed that special attention should be given to the question of dust in the atmosphere above Mount Wilson. It has fortunately turned out, as will be shown later, that the presence of any appreciable amount of dust in the air above the mountain is so exceptional a phenomenon as to constitute no important objection to Mount Wilson as an observatory site.

After a brief statement regarding the conditions found at Mount Wilson had been presented to the Executive Committee of the Carnegie Institution, in April 1904, they decided to make a grant of a sum sufficient to provide for the erection and use of the Snow telescope on the mountain. The Yerkes Observatory loaned the telescope, and the University of Chicago provided the salaries of some of the observers. The work accomplished on the mountain since this grant was made has been sufficient to serve as a reliable basis for estimates on the cost of a large solar observatory, besides giving valuable experience regarding the necessary methods and cost of construction under the unusual conditions existing at the summit of a mountain nearly 6,000 feet in height. In view of their bearing on the question of a solar observatory, I have accordingly included in my report some remarks on the principal obstacles encountered and overcome in the construction of buildings and the transportation of instruments and materials.

REQUIREMENTS OF A SITE FOR A SOLAR OBSERVATORY

It is desirable to recapitulate here the purposes and plans for a solar observatory which were given at some length in *Year Book* No. 2 of the Carnegie Institution. At the outset, it should be stated that the term "solar observatory" is used here in a broad sense, since it is not intended to exclude from the program certain investigations of stars which are of fundamental importance in any general study of the problem of stellar evolution. For the Sun is a star, comparable in almost every respect with many other stars in the heavens, and

rendering possible, through an intimate knowledge of its own phenomena, the solution of some of the most puzzling questions in the general problem of stellar evolution. Conversely, however, the stars are suns, and if we would know the past and future conditions of the Sun, we must examine into the physical condition of stars which represent earlier and later stages of development. It will be seen that there is ample ground for the inclusion in the equipment of a solar observatory of certain instruments especially designed for the study of stellar problems.

The plan of work proposed for the observatory, as outlined in *Year Book* No. 2, includes the following classes of observations:

1. Frequent measurements of the heat radiation of the Sun, to determine whether there may be changes during the sun-spot cycle in the amount of heat received from the Sun by the Earth and in the relative radiation of the various portions of the solar surface.

2. Studies of various solar phenomena, particularly through the use of powerful spectroscopes and spectroheliographs.

3. Photographic and spectroscopic investigations of the stars and nebulæ with a very powerful reflecting telescope, for the principal purpose of throwing light on the problem of stellar evolution.

The present opportunity for important advances in these three departments of research is very unusual. Since the publication of *Year Book* No. 2, Dr. Langley has offered reasons to believe that an actual change in the amount of heat emitted by the Sun occurred in March 1903. It is hardly necessary to say that a change in the intensity of the Sun's heat, if actually established, might have a most important bearing upon many questions relating to the Earth, and, at the same time, be of capital interest in its relationship to the problem of the solar constitution. Through the force of circumstances, Dr. Langley's observations have been made under the very unfavorable conditions which obtain at Washington. If they could be continued at a considerable altitude, at a point above the denser and more fluctuating region of the Earth's atmosphere, the question as to what changes actually occur in the solar radiation could doubtless be answered in a thoroughly satisfactory manner.

In the study of the phenomena of the Sun's surface and atmosphere we again enter a remarkably fruitful field of research. Within the

past few years the instruments available for work in this field have been greatly developed, and now only await application on a large scale in order to secure a great number of new results which have hitherto been entirely out of reach. But even if the means were available for supplying the necessary instruments to existing observatories, they could not be successfully employed without atmospheric conditions much superior to those at present available. In work of this nature, success depends upon the perfect definition of the solar image and the absence of those disturbances from which the amosphere at existing observatories is almost never free. For this work, therefore, an elevated station in a region of great atmospheric calm is absolutely essential. Furthermore, the site must be free from the disturbing factors which frequently prevent good observations from being obtained on mountain summits.

In the third class of investigations required to complete the program of a properly equipped solar observatory, similar possibilities of advance exist. Within the past few years the remarkable advantages of the reflecting telescope have been demonstrated. It now only remains to construct a large and powerful instrument of the type shown by these experiments to promise success. With such an instrument, immense new fields of investigation of the highest importance in their bearing on the problem of stellar evolution could be immediately occupied. Here again, however, the unfavorable atmospheric conditions at almost all existing observatories would render the construction of a large telescope almost useless. To be successful, such an instrument must be erected at a site where the night-seeing is nearly perfect, the sky clear and transparent, and the average wind velocity very low. Under such conditions, a properly constructed telescope of large aperture would undoubtedly yield results greatly surpassing those hitherto obtained.

These considerations are sufficient to define the general character of a site suitable for a well-equipped solar observatory. There are other points, however, which must be taken into account. A solar observatory provided with an outfit of instruments, and then left to do its work without the possibility of improvement or change, could never attain the best results. On the contrary, it must have the means of producing new types of instruments and modifying old ones, as

the development of the work may suggest. In other words, a shop completely equipped with all appliances necessary for the most refined construction of both the mechanical and optical parts of instruments should form an integral part of a solar observatory. A shop of this kind cannot be conducted without great difficulty and expense, if far removed from large cities and other sources of supply. This is only one of many reasons which would render it desirable to select an observatory site within easy reach of the facilities afforded by a large city.

In his recommendation for the establishment of an observatory for the purpose of determining whether the heat radiation of the Sun undergoes change, Dr. Langley pointed out the desirabliity of making the observations at a height of some 20,000 feet above sea-level. Apart from the excessive difficulty and expense of conducting an observatory at such an elevation, which are best appreciated by those who have worked at great altitudes, the inaccessibility of high mountain peaks would stand in the way of such an undertaking. But it nevertheless might have been carried out, at a somewhat lower altitude, if the recent development of Dr. Langley's work at Washington had not indicated that the great mass of observations could undoubtedly be made to good advantage at a much lower station. The increasing perfection of the observational method has, indeed, permitted fairly good results to be obtained under the very unfavorable conditions which exist at Washington. Nevertheless, it by no means follows that Dr. Langley's purpose could be accomplished at such a point. The humidity of our atmosphere is a most serious obstacle in this particular work, since the solar heat is very subject to absorption by water vapor. It is therefore desirable to establish the instruments a least a mile above the dense and disturbed layers of the atmosphere which lie near the sea-level. Certain problems connected with the investigation may render it desirable to make some of the observations at a higher altitude, reaching from 12,000 to 15,000 feet. We conclude, therefore, that the principal work should be done at a station having an elevation of 5,000 to 6,000 feet, in a dry climate, where the weather is continuously clear over long periods of time. The work at higher altitudes, if needed at all, could in all probability be completed in two or three summers by expeditions equipped with

a portable outfit erected at an altitude of from 12,000 to 15,000 feet. It would thus be convenient to have the principal station at a lower altitude, not far removed from accessible mountains of this considerable elevation. It would be inadvisable, for reasons which it is hardly necessary to specify, to establish the principal station at an altitude much greater than 6,000 feet.

POSITION AND NATURAL RESOURCES OF MOUNT WILSON

From a meteorological standpoint, the state of California may naturally be divided into three parts. In the northern region the rainfall is very considerable, much cloudiness prevails, and in almost all respects the conditions are very unfavorable for astronomical work. The central region, which may be considered to extend as far south as Point Concepcion, is favored with much better weather conditions, best exemplified at the Lick Observatory, on Mount Hamilton, where a high average of night-seeing is maintained during a large part of the year. Except for the frequent winds at night, which interfere with some classes of work, Mount Hamilton might be regarded as an almost ideal observatory site, at least for night observations. For solar work it may not be superior to certain stations in the eastern part of the United States, because of the excessive radiation from the heated slopes of the mountain, which is almost devoid of trees near the summit.

In the southern part of California the climatic conditions are decidedly different from those which prevail in the two other sections of the state. The much lighter rainfall is naturally associated with fewer clouds, a remarkably steady barometer, and very light winds. During a part of the year the fog rolls in from the ocean and covers much of the San Gabriel Valley during the night. But these fog-clouds rarely attain elevations exceeding 3000 feet, except when storm conditions prevail during the winter months. The mountains of the Sierra Madre range rise high above the fog, and during a great proportion of the year they enjoy practically continuous sunshine. During the summer months the sea breeze blows for a large part of the day, but it attains only a low velocity, which decreases in passing from the valley to the mountain tops and in going inward from the coast.

Mount Wilson is one of many mountains that form the southern boundary of the Sierra Madre range. Standing at a distance of thirty miles from the ocean, it rises abruptly from the valley floor, flanked only by a few spurs of lesser elevation, of which Mount Harvard is the highest. Except for a narrow saddle, Mount Wilson is separated from Mount Harvard by a deep cañon, the walls of which are very precipitous. Farther to the west, beyond the saddle leading to Mount Harvard, the ridge of Mount Wilson forms the upper extremity of Eaton Cañon, which leads directly to the San Gabriel Valley. East and north of Mount Wilson lies the deep cañon through which flows the west fork of the San Gabriel River, and beyond this rises a constant succession of mountains, most of them higher than Mount Wilson, which extend in a broken mass to the Mojave Desert. The Sierra Madre range forms the northern boundary of the San Gabriel Valley, which is further protected toward the east from the desert by the high peaks of the San Bernardino range. Through the Cajon Pass, where the Atchison, Topeka & Santa Fé Railroad enters the valley, winds from the desert frequently blow, bringing vast quantities of dust, which sometimes diffuses through the lower air over the entire valley. This dust but rarely reaches an elevation as great as that of Mount Wilson, though I have seen a few windstorms that carried the dust of the desert directly over the Sierra Madre range and into the valley below.

For the most part, the readily accessible mountains on the southern boundary of the Sierra Madre range have few trees near the summit, and enjoy but small supplies of water. Mount Wilson is remarkable in having a fine growth of trees covering its summit, and in possessing within easy reach of its highest point several large springs of water, which afford a good supply even during very dry seasons.

In a dry country the question of a pure and permanent supply of water is of paramount importance. It is therefore desirable to give more definite information of the springs near the summit of Mount Wilson. Some of these are located at Strain's Camp, where, for many years, they have supplied the necessities of summer visitors, who frequently occupy tents here for considerable periods of time. Two wells have been dug at Strain's Camp, and these are regarded as excellent sources of pure water.

In accordance with the terms of the lease of the property at present occupied as an observatory site on Mount Wilson, the water rights on the mountain are to be equally divided between the owners of the property and the occupants of the observatory site. It seems probable that the wells at Strain's Camp, if properly developed, would supply the purposes of a large observatory. If not, more water could easily be developed on the mountain; it may appear desirable to obtain water from a stream in one of the neighboring cañons, about 1000 feet below. The expense of pumping to this height would be great, and the stream can be relied upon as a never-failing source of water. A water-tunnel on the south face of the mountain has been reserved by the owners of the property for the purpose of supplying Martin's Camp, and is not included in the equal division of the remaining water rights. A method of securing more water, which could undoubtedly be employed with advantage, would be through the use of large storage tanks, in which water could be collected during the rainy season, either by pumping from the overflowing wells or by catching the rain as it falls on roofs or other large surfaces provided for the purpose.

TRANSPORTATION AND CONSTRUCTION

Much granite is available on Mount Wilson for the purpose of construction, but in the portion of the mountain selected for the observatory site it is not so easily obtained as might be wished. This is due to the fact that much of the granite is decomposed, and consequently too soft for building purposes. The hard and the decomposed granites occur together, so that if a quarry is started at a point where plenty of hard granite seems to be present, it sometimes happens that the supply is soon exhausted, leaving only decomposed granite below. Men experienced in matters of this kind have been quite unable to judge whether selected spots could be relied upon to furnish a good supply of hard granite. This fact greatly increases the expense of constructing stone piers, since quarries may have to be abandoned after having been opened at considerable cost. However, some abundant sources of excellent stone can be rendered easily accessible by the extension of roads constructed for work now in progress.

Numerous fallen trees on Mount Wilson, which are not yet greatly decayed, will furnish an abundant supply of firewood for many years to come. They cannot be depended upon, however, to yield any wood for building purposes, and as the living trees may not be destroyed, all lumber must be taken to the summit of the mountain from Pasadena. This raises the question of transportation over the mountain trail—a matter of vital importance in constructing an observatory. The "Toll Road" or "New Trail," which extends from the summit of the mountain to the foot of Eaton Cañon, is well adapted for all ordinary packing with animals, though it is much too narrow to permit wagons to pass over it. At present, all except the heaviest articles are taken to the summit of the mountain by means of burros and pack-mules, each of which can carry a load ranging from 80 to 200 pounds. It is evident that transportation of building materials by this means must be very slow and expensive, since the trail is nine miles in length to the foot of Eaton Cañon, six and one-half miles distant by road from Pasadena. But, as compared with most mountains, Mount Wilson is unusually accessible from cities, Pasadena being so close at hand, and Los Angeles, with its large sources of supply, being only nine miles farther away.

For transporting heavy castings and other similar articles, we have found it necessary to construct a special four-wheel carriage, two feet in width. On this loads of a thousand pounds have been taken to the summit without difficulty. By widening the trail to six feet, the heaviest castings required for a solar observatory probably could be transported.

WEATHER

So far as cloudiness is concerned, the records of the Weather Bureau at Los Angeles are of comparatively little value for our present purposes. The fog rolls in from the ocean night after night, and sometimes hangs over Los Angeles throughout the day during the winter season. But Mount Wilson reaches far above this layer of clouds, and thus frequently enjoys sunshine when the valley below is completely covered. Our daily percentage record of cloudiness, beginning on April 18, 1904, may be found in the following table. A dash signifies that no observation was made.

There were many days which were cloudy at the time of observation, but nevertheless suitable at other hours for solar work. Adding these to the record, it may be said that the actual number of days on

CLOUDINESS

Day of Month	April		May		June		July		August	
	8 A. M.	6 P. M.	8 A. M.	6 P. M.	8 A. M.	6 P. M.	8 A. M.	6 P. M.	8 A. M.	6 P. M.
1	...	...	100	20	75	75	0	0	0	0
2	...	...	0	5	5	0	0	0	0	0
3	...	...	0	0	0	0	0	0	90	10
4	...	...	0	0	0	0	0	0	5	25
5	...	...	0	0	0	0	0	0	0	0
6	...	...	0	0	0	0	0	0	30	100
7	...	...	0	0	0	0	0	—	40	60
8	...	...	0	0	0	0	0	10	50	40
9	...	...	70	85	0	0	0	—	0	0
10	...	...	5	5	0	0	—	—	0	0
11	...	...	0	0	0	0	0	0	0	0
12	...	...	0	0	0	0	0	0	30	50
13	...	...	0	0	0	0	0	0	20	20
14	...	...	0	0	0	0	0	0	80	80
15	...	...	0	0	0	25	0	0	0	0
16	...	...	0	0	5	0	0	—	0	5
17	..	...	0	0	0	0	—	—	0	0
18	...	...	0	0	0	10	—	0	10	5
19	100	100	0	0	0	0	0	0	0	0
20	50	5	0	75	0	0	5	5	0	0
21	0	0	25	5	0	0	—	50	0	5
22	70	100	0	50	0	0	45	5	75	80
23	0	0	5	5	0	0	35	5	20	30
24	0	5	0	0	0	0	5	10	60	5
25	75	0	100	100	0	0	35	15	5	5
26	100	100	100	0	0	0	20	15	5	5
27	0	100	0	0	0	0	12	7	0	5
28	80	100	0	0	0	0	15	5	0	5
29	0	0	10	75	0	0	10	5	0	5
30	0	—	0	0	0	0	5	—	5	80
31	...	...	0	0	...	...	—	—	80	5

which observations could be made amount to 132 out of 135. The long periods of perfectly clear weather, permitting observations of the Sun to be made without interruption from day to day, should prove of the greatest importance in the study of many solar problems which require daily observations for their solution. From the records so far obtained, it seems probable that observations of the Sun could be made at Mount Wilson on more than 300 days in a year. In Los Angeles, during the past twenty-three years, the average number of "clear" days in the year is 317.

CLOUDINESS

Day of Month	September		October		November		December	
	8 A. M.	6 P. M.	8 A. M.	6 P. M.	8 A. M.	6 P. M.	8 A. M.	6 P. M.
1	0	0	15	10	80	90	100	100
2	0	0	0	0	0	0	100	100
3	0	5	15	60	0	0	0	5
4	0	60	80	50	5	40	80	25
5	0	40	0	50	3	60	5	3
6	5	0	100	100	20	20	0	0
7	0	0	100	100	0	30	5	3
8	0	5	100	—	30	0	5	3
9	10	60	—	—	0	5	5	95
10	0	0	—	10	15	5	0	0
11	0	5	60	40	3	0	10	25
12	0	60	0	0	80	70	—	0
13	0	5	0	0	80	5	10	70
14	0	0	0	0	7	75	20	50
15	3	15	20	10	95	50	40	40
16	60	3	0	0	5	0	5	0
17	50	40	0	0	0	0	10	3
18	20	5	0	0	15	0	5	0
19	40	50	0	0	5	0	0	5
20	70	40	0	0	3	10	15	50
21	75	95	0	0	60	15	90	95
22	100	30	0	0	10	3	100	10
23	100	100	0	0	0	5	90	5
24	100	100	0	0	—	—	100	100
25	100	100	0	3	—	15	5	5
26	100	98	0	0	50	30	—	—
27	—	—	0	0	70	10	—	—
28	—	0	0	3	5	5	0	5
29	0	40	0	15	0	3	60	0
30	0	3	50	3	20	40	3	95
31	..	..	0	0	..	..	100	30

The cloudiness in July and August was due almost entirely to thunderstorms over the desert to the north and east. The clouds rarely reached our zenith and almost never interfered with the regular solar observations (cf. table of Seeing).

HUMIDITY

The question of humidity is of special importance in connection with the measurement of the solar constant, since water-vapor in the atmosphere absorbs very strongly the solar heat. The results obtained with a standard sling psychrometer, Weather Bureau pattern, are given in the following table:

RELATIVE HUMIDITY AT MOUNT WILSON

Day of Month	April		May		June		July		August	
	8 A. M.	6 P. M.	8 A. M.	6 P. M.	8 A. M.	6 P. M.	8 A. M.	6 P. M.	8 A. M.	6 P. M.
1	..	..	100	80	40	36	34	24	22	27
2	..	..	57	80	39	39	23	38	22	33
3	..	..	34	66	39	41	30	32	29	30
4	..	..	46	49	24	25	38	56	32	40
5	..	..	38	38	20	24	34	42	40	43
6	..	..	29	40	27	42	45	43	34	43
7	..	..	36	31	29	41	49	—	—	76
8	..	..	18	22	23	29	33	18	64	58
9	..	..	15	24	20	34	42	—	41	46
10	..	..	22	41	21	21	—	—	28	35
11	..	..	36	38	19	17	57	24	30	33
12	..	..	36	54	—	—	29	40	53	65
13	..	..	41	40	—	43	46	22	61	64
14	..	..	32	64	52	42	25	25	67	72
15	..	..	23	29	32	30	30	44	54	45
16	..	..	27	19	34	30	50	—	37	36
17	..	..	34	38	33	24	—	—	40	30
18	—	85	25	66	32	41	—	16	34	45
19	100	100	46	64	31	42	22	24	32	46
20	96	73	41	100	56	72	21	19	23	40
21	60	79	42	45	45	53	—	42	37	33
22	92	98	28	43	40	43	24	34	58	85
23	63	65	37	32	44	54	35	35	79	79
24	60	50	38	50	42	32	31	26	77	56
25	29	68	100	100	30	27	33	50	69	58
26	100	100	100	48	28	25	38	27	67	37
27	100	100	37	38	27	36	31	50	40	43
28	100	100	38	33	42	35	30	32	43	33
29	46	36	23	15	35	30	50	46	21	29
30	58	71	14	47	30	37	38	—	27	14
31	..	..	30	35	..	..	—	—	14	31
Means...	77		43		34		33		43	

The marked dryness of the atmosphere on Mount Wilson during the summer months may be best appreciated by comparing these results with those obtained by the Weather Bureau at Washington during the corresponding period.

RELATIVE HUMIDITY AT WASHINGTON

Month	Mean	Maximum[1]	Minimum[1]
April	63.0	100	30
May	65.6	97	41
June	77.8	98	54
July	60.6	99	51
August	78.0	95	59

[1] Mean maximum and mean minimum humidity not determined.

RELATIVE HUMIDITY AT MOUNT WILSON

Day of Month	September		October		November		December	
	8 A. M.	6 P. M.	8 A. M.	6 P. M.	8 A. M.	6 P. M.	8 A. M.	6 P. M.
1	26	29	47	38	62	46	100	100
2	38	24	49	48	84	89	100	100
3	31	38	42	37	72	83	70	57
4	43	43	60	63	71	53	63	49
5	31	22	69	100	49	53	58	52
6	22	25	100	100	63	57	53	47
7	20	18	100	100	38	57	59	43
8	30	40	100	—	52	35	47	59
9	41	39	—	—	37	51	57	51
10	53	48	—	86	56	61	12	14
11	48	45	100	100	54	26	12	17
12	60	63	53	56	48	44	19	42
13	57	63	53	47	61	30	48	43
14	51	28	56	60	22	21	33	30
15	37	43	67	77	28	56	18	—
16	41	27	67	55	57	50	—	21
17	32	66	49	54	72	41	20	30
18	54	78	44	52	71	81	26	35
19	60	37	59	66	59	35	42	28
20	46	49	67	68	60	34	29	34
21	32	67	33	31	69	29	42	65
22	93	93	20	27	59	57	100	84
23	94	100	22	34	55	47	91	95
24	100	100	24	28	—	—	100	100
25	100	100	23	27	—	32	81	81
26	100	80	26	34	65	41	—	—
27	—	—	29	47	31	29	—	—
28	—	64	46	71	53	50	56	45
29	60	52	41	66	36	40	50	56
30	57	47	66	81	49	62	38	90
31	..	.	78	40	..	..	100	83
Means	52		57		51		55	

The chief of the Weather Bureau has also kindly sent the following data as to the mean relative humidity at Washington for the remainder of the year. The two values refer to the results for 8 A. M. and 8 P. M., respectively. September, 84.8, 78.2; October, 80.8, 71.7; November, 76.2, 66.9; December, 77.8, 70.6.

TEMPERATURE

From March 25 to April 15 the temperature was recorded on a self-registering thermometer. After April 15 this record was supplemented by observations of maximum and minimum thermometers.

The results are given (in degrees Fahrenheit) in the following table. As bearing upon certain classes of night observations, the range of temperature between 8 P. M. and 4 A. M. is also included.

TEMPERATURE

Day of Month	April			May			June			July			August		
	Maximum	Minimum	Range 8 P. M. to 4 A. M.	Maximum	Minimum	Range 8 P. M. to 4 A. M.	Maximum	Minimum	Range 8 P. M. to 4 A. M.	Maximum	Minimum	Range 8 P. M. to 4 A. M.	Maximum	Minimum	Range 8 P. M. to 4 A. M.
1	..	..	..	49	32	8	76	46	4	93	57	5	83	57	3
2	..	..	..	56	27	4	82	53	1	81	58	5	90	63	5
3	..	..	..	64	39	4	89	55	2	79	56	8	91	66	3
4	..	..	..	70	42	5	80	56	7	80	56	3	89	66	5
5	..	..	..	79	45	7	81	59	5	77	54	6	91	66	2
6	..	..	..	80	55	2	78	57	6	75	55	6	92	68	2
7	..	.	..	84	51	8	75	48	9	—	—	5	—	—	6
8	..	..	..	84	55	7	79	52	3	76	52	6	83	64	3
9	..	..	..	85	57	5	83	57	4	—	—	2	87	60	6
10	.	..	..	86	57	5	82	61	5	—	—	2	86	64	4
11	..	..	.	79	57	2	87	60	4	82	57	5	87	63	4
12	..	..	.	85	54	2	—	—	5	86	56	4	88	62	5
13	..	..	..	75	56	4	—	—	5	89	59	3	85	64	3
14	..	..	..	79	50	6	77	57	6	83	63	7	83	60	3
15	..	..	..	85	53	4	80	54	4	75	57	4	86	60	2
16	..	..	..	82	57	4	79	53	5	72	50	4	87	60	5
17	..	..	..	79	56	5	81	56	5	84	63	6	87	62	5
18	53	37	6	75	49	4	83	55	8	93	61	1	88	63	4
19	42	35	3	56	36	8	79	59	5	95	66	3	93	64	5
20	41	23	6	74	45	1	73	51	7	91	69	1	90	65	5
21	49	34	1	78	50	5	86	50	1	—	—	2	86	59	8
22	46	32	5	81	51	4	83	55	2	82	62	4	77	60	2
23	49	25	9	78	56	3	78	55	6	89	62	2	78	56	2
24	63	36	3	69	54	4	78	53	6	91	66	3	81	56	0
25	64	40	4	52	38	10	87	56	3	88	68	4	85	61	2
26	43	29	8	53	35	3	88	62	1	82	66	3	89	62	3
27	43	23	4	69	41	0	85	63	5	88	62	2	87	64	2
28	44	31	0	75	49	5	79	57	4	83	68	5	87	62	2
29	63	36	2	71	50	5	79	55	3	83	62	7	87	62	2
30	65	41	5	70	46	6	78	53	5	—	—	3	81	59	4
31	—	—	—	77	45	4	—	—	—	—	—	4	82	59	3
Means	51.2	32.7	4.3	73.5	48.0	4.7	80.9	55.3	4.5	84.0	60.2	4.0	86.2	61.9	3.5
Daily r'ge	18.5		....	25.5		....	25.6		....	23.8		....	24.3		....

TEMPERATURE

Day of Month	September			October			November			December		
	Maximum	Minimum	Range 8 P. M. to 4 A. M.	Maximum	Minimum	Range 8 P. M. to 4 A. M.	Maximum	Minimum	Range 8 P. M. to 4 A. M.	Maximum	Minimum	Range 8 P. M. to 4 A. M.
1	87	60	3	79	60	5	75	48	2	60	38	4
2	90	66	2	83	54	5	63	42	7	48	36	4
3	91	66	3	84	55	4	78	40	3	59	33	4
4	89	63	5	87	59	2	71	46	3	64	35	2
5	93	65	5	66	47	3	70	45	2	60	29	7
6	97	67	4	56	37	8	72	46	1	63	31	3
7	97	69	4	49	39	2	80	49	3	64	33	2
8	87	68	5	—	—	0	84	49	2	61	37	1
9	87	63	7	—	—	2	82	51	2	66	37	2
10	83	59	7	67	—	2	82	52	3	74	41	1
11	83	59	3	56	43	7	76	47	5	72	42	4
12	86	57	3	72	36	4	69	49	7	68	37	5
13	85	58	4	74	45	4	72	42	3	70	36	4
14	87	58	1	71	48	5	72	48	1	66	44	1
15	83	59	2	67	43	5	64	44	3	—	44	5
16	81	59	3	75	43	4	74	38	6	66	52	—
17	74	50	4	75	39	6	73	47	2	65	41	2
18	71	44	5	77	42	2	67	45	3	67	49	2
19	78	49	2	82	46	3	76	42	5	69	42	3
20	76	50	3	84	46	4	78	47	2	67	50	2
21	72	52	3	81	50	4	80	50	3	60	39	3
22	51	41	7	83	53	4	73	50	3	44	32	5
23	51	39	4	79	54	4	72	49	2	45	33	2
24	54	41	2	82	54	2	—	—	1	41	35	2
25	52	43	1	82	54	3	72	—	1	50	31	5
26	52	36	6	78	52	3	77	51	3	—	—	3
27	—	—	2	74	47	6	73	52	2	—	—	3
28	75	—	3	72	47	3	73	50	3	56	34	5
29	82	50	3	67	47	1	75	49	2	59	44	3
30	81	52	5	67	46	2	72	45	2	54	38	4
31	..	..	..	75	43	3	..	..	..	43	33	4
Means	78.4	55.1	3.7	73.9	47.5	3.6	74.0	46.9	2.9	60.0	38.1	3.1
Daily range	23.3			26.4			27.1			21.9		

ATMOSPHERIC PRESSURE

No complete barometric record has been kept, since this did not seem of special importance in connection with the work. Nevertheless, an aneroid barometer has been read twice daily since July 13. The maximum and minimum readings recorded up to September 1 differed by only 0.22 inch.

WIND MOVEMENT

With such uniformity of atmospheric pressure, it might naturally be anticipated that the wind movement would be low. The results of anemometer readings (in miles), made with an instrument of the standard Weather Bureau pattern, are shown in the following table. The "day" results give the total movement from 8 A. M. to 6 P. M.; the "night" results give the total movement from 6 P. M. to 8 A. M.

Day of Month	April		May		June		July		August	
	Day	Night	Day	Night	Day	Night	Day	Night	Day	Night
1	...	...	99	75	120	175	55	145	43	95
2	...	...	49	165	71	141	71	102	50	72
3	...	...	52	118	44	185	47	71	45	61
4	...	...	29	88	44	114	34	41	46	48
5	...	...	43	91	61	47	30	54	60	41
6	...	...	30	62	78	108	43	78	43	49
7	...	...	34	128	59	155	40	44	47	80
8	...	...	43	106	55	146	49	85	33	63
9	...	...	38	44	50	66	44	83	43	60
10	...	...	44	133	52	86	43	77	62	78
11	...	...	54	110	43	49	49	93	49	81
12	...	...	35	101	32	75	31	74	58	122
13	...	...	41	80	30	95	42	81	59	71
14	..	...	39	95	23	133	42	59	44	73
15	...	...	72	114	49	61	39	68	44	55
16	...	. .	42	110	56	80	33	50	45	80
17	...	...	46	81	40	58	40	70	44	60
18	. .	...	50	56	37	56	32	62	26	55
19	140	188	105	136	30	47	38	78	37	58
20	62	70	45	48	36	74	63	91	36	100
21	35	144	63	71	56	90	37	60	39	55
22	40	42	66	82	37	56	43	37	63	104
23	47	120	67	97	74	103	44	85	63	113
24	32	109	69	79	44	86	60	64	55	101
25	70	63	63	136	52	51	44	56	50	64
26	91	191	33	60	55	59	39	71	35	66
27	44	51	50	58	75	51	47	80	56	84
28	33	34	21	101	42	52	41	57	54	107
29	50	101	62	73	37	60	56	65	55	56
30	47	63	51	80	56	46	57	131	57	69
31	...	...	57	151	...	...	53	97	38	54
Total	691	1,176	1,592	2,929	1,538	2,605	1,386	2,309	1,479	2,275
Means	57.6	96.0	51.3	94.5	51.3	86.6	44.7	74.5	47.7	73.4
Hourly means	5.8	6.8	5.1	6.7	5.1	6.2	4.5	5.3	4.8	5.2

It appears from these results that the average wind movement is exceptionally low. The importance of this fact in its indication of a uniform atmosphere, and in connection with astronomical photog-

WIND MOVEMENT

Day of Month	September		October		November		December	
	Day	Night	Day	Night	Day	Night	Day	Night
1	34	76	43	60	61	71	27	81
2	51	30	39	32	42	123	36	70
3	52	67	38	58	44	130	34	82
4	40	70	50	81	55	47	42	137
5	49	71	58	100	38	88	80	120
6	50	97	44	125	41	95	40	141
7	51	48	85	56	38	64	42	78
8	50	88	—	129	48	97	36	62
9	47	68	—	—	41	144	38	66
10	35	54	—	—	36	151	66	106
11	37	53	68	86	32	45	37	59
12	42	44	40	102	35	148	65	165
13	46	73	41	52	25	53	38	154
14	47	71	53	83	49	46	38	56
15	66	86	40	117	35	105	53	69
16	73	81	50	79	37	71	39	68
17	51	101	89	207	56	93	31	56
18	33	91	50	139	44	92	31	53
19	38	83	65	128	39	48	33	64
20	36	71	47	194	29	51	37	51
21	80	52	37	125	39	73	85	74
22	37	110	46	45	59	110	64	101
23	83	84	45	44	65	141	36	46
24	72	161	36	74	—	—	82	113
25	70	163	38	66	—	—	66	142
26	64	101	36	55	53	91	—	—
27	—	—	42	49	46	91	—	—
28	—	—	53	62	45	47	37	69
29	64	158	64	94	47	73	57	48
30	58	53	39	66	65	165	122	146
31	..	..	42	73	..	..	60	200
Means	52.0	82.3	49.2	89.0	44.4	91.2	50.1	92.3
Hourly means	5.2	5.9	4.9	6.4	4.4	6.5	5.0	6.6

raphy, will be appreciated by astronomers. The shaking of a large instrument by the wind is frequently so serious as to reduce greatly the quality of astronomical photographs obtained in windy weather. At Mount Wilson, where a dead calm is an exceedingly common occurrence, all of the most exacting requirements of astronomical photography are completely realized.

TRANSPARENCY OF THE ATMOSPHERE

I have previously alluded to the dust-storms which sometimes enter the San Gabriel Valley through the Cajon Pass from the Mojave Desert, and those much rarer storms in which the dust is carried by

the wind completely over the Sierra Madre Mountains. In the more common form of dust-storm (the so-called "Santa Ana") the dust enters the valley in a fairly well-defined mass and proceeds westward along the cañon of the Santa Ana River. In approaching the coast it spreads over a large area and diffuses itself with tolerable uniformity through the lower atmosphere. I have seen from Mount Wilson a dust-storm in the region of Riverside, which in twenty-four hours had spread itself over Los Angeles and Pasadena. When it reached this part of the valley there was almost no wind, and the dust seemed to diffuse itself through the air. Such storms sometimes completely hide the Sierra Madre Mountains from observers in Pasadena. Fortunately they are almost always confined to the lower atmosphere, and do not appreciably affect the transparency of the sky above Mount Wilson, where daily observations show that the transparency of the day and night sky are very satisfactory.

SEEING

Systematic tests of the definition of the solar image have been made on Mount Wilson with a telescope of 3¼ inches aperture, with an eyepiece giving a power of about 100 diameters. At first the character of the seeing was rated on a scale of 5; but it soon appeared that a scale of 10 would be preferable under the existing conditions. Accordingly, the seeing as recorded in the following table is given on a scale of 10. Seeing 8, which is so frequently obtained during the early morning hours, represents a sharply defined image of the Sun, showing the granulation and the details of the spots with great distinctness, and indicating practically no trembling at the limb. Such seeing occurs at the Yerkes Observatory only occasionally, although that observatory seems to be better situated than many other institutions for work on the Sun.

An examination of the table will show that the seeing is best during the early morning hours, although the image is frequently very good in the late afternoon. Shortly after sunrise the Sun's limb is serrated, but this effect becomes less and less marked as the Sun's altitude increases. Usually, at this time in the morning, the atmosphere is almost perfectly calm and cloudless. The seeing usually improves and reaches a maximum, where it remains for some time. The effect

SEEING

April	Hour of Observation								
	6	7	8	9	10-2	3	4	5	6
1...	–	5	5	4	4	4	5	4	–
2...	–	–	5	6	5	5	4	4	–
3...	–	5	6	7	6	–	–	–	–
4...	–	5	6	7	6	3	5	–	–
5...	–	8	8	8	7	–	8	8	6
6...	–	6	7	6	4	7	7	5	–
7...	–	9	8	7	7	6	7	7	5
8...	–	–	7	–	–	–	–	–	–
9...	–	5	5	4	4	4	5	4	–
10...	6	7	7	6	6	7	–	–	–
11...	7	9	7	6	6	7	–	–	–
12...	–	7	7	6	–	–	–	–	–
13...	–	8	8	7	–	–	7	8	7
14...	–	6	6	6	5	5	4	–	–
15...	–	–	–	–	–	–	–	–	–
16...	–	5	–	5	–	–	–	4	–
17...	–	–	5	4	4	6	6	–	–
18...	–	6	7	–	–	–	–	–	–
19[1]..	–	–	–	–	–	–	–	–	–
20...	–	6	5	–	–	–	–	–	–
21...	–	6	7	6	5	4	6	6	–
22[1]..	–	–	–	–	4	–	–	–	–
23...	–	–	5	4	4	4	4	–	–
24...	–	–	7	6	5	5	6	6	7
25...	–	–	–	–	4	4	4	4	–
26[2]..	–	–	–	–	–	–	–	–	–
27[1]..	–	6	4	–	–	–	–	–	–
28[1]..	–	6	–	–	–	–	–	–	–
29...	–	7	6	6	4	5	6	–	–
30...	6	7	4	4	4	4	6	–	–

May	Hour of Observation								
	6	7	8	9	10-2	3	4	5	6
1[2]..	–	–	–	–	–	–	–	–	–
2...	–	5	4	4	3	2	2	2	–
3...	–	7	6	7	5	4	6	–	–
4...	–	6	6	5	4	5	5	–	–
5...	–	7	–	–	4	5	5	4	–
6...	7	8	6	–	–	–	–	–	–
7...	5	4	4	4	4	4	4	4	–
8...	–	7	7	5	–	–	–	–	–
9[3]..	–	–	5	–	–	–	–	–	–
10...	–	5	6	5	5	–	–	–	–
11...	–	6	5	5	4	–	–	–	–
12...	–	–	–	–	–	–	–	–	–
13...	–	–	–	–	–	–	–	–	–
14...	–	–	–	–	–	–	–	–	–
15...	–	5	6	5	–	–	–	–	–
16...	6	–	–	7	7	–	6	6	–
17...	–	–	–	–	–	–	–	–	–
18...	–	–	–	–	–	–	–	–	–
19...	–	–	–	–	–	–	–	–	–
20...	–	–	–	–	–	–	–	–	–
21...	–	–	–	–	–	–	5	–	–
22...	–	6	7	8	–	4	–	–	–
23...	–	6	–	6	5	–	6	–	–
24...	–	8	–	6	6	–	5	–	–
25[1]..	–	–	–	–	–	–	–	–	–
26[1]..	–	–	–	–	–	–	–	8	–
27...	–	8	7	6	6	7	7	8	–
28...	–	8	8	–	7	–	8	–	7
29...	–	9	8	8	8	–	–	–	–
30...	–	7	8	8	7	6	6	7	–
31...	–	6	7	–	6	6	7	–	–

June	Hour of Observation								
	6	7	8	9	10-2	3	4	5	6
1[3]...	–	–	–	–	–	–	6	–	–
2...	–	–	7	6	6	6	6	6	–
3...	–	6	6	6	6	6	6	6	–
4...	–	6	–	7	5	–	–	–	–
5...	–	8	8	–	7	–	6	–	4
6...	–	9	9	8	7	7	–	–	–
7...	–	7	–	–	4	4	6	–	7
8...	–	8	8	–	6	–	–	7	6
9...	–	9	8	6	6	–	6	7	–
10...	–	9	8	–	7	–	–	7	–
11...	–	9	8	–	6	–	8	7	9
12...	–	9	–	–	–	–	–	–	–
13...	–	–	–	–	–	–	–	–	–
14...	–	–	–	–	–	–	–	8	–
15...	–	9	8	7	6	–	7	7	–
16...	–	9	8	–	7	–	8	7	8
17...	–	9	9	7	7	5	–	7	8
18...	–	8	7	–	5	–	7	8	7
19...	–	9	6	6	4	–	–	7	–
20...	6	6	–	6	5	7	7	–	6
21...	–	6	6	–	5	–	6	–	6
22...	8	–	8	6	6	6	7	8	8
23...	–	8	7	–	7	–	7	8	–
24...	–	9	8	–	6	–	7	8	–
25...	8	9	7	–	7	–	7	7	–
26...	–	9	8	8	6	–	6	6	–
27...	–	8	8	7	7	–	–	5	6
28...	8	8	–	–	6	–	6	6	–
29...	8	9	–	6	6	–	–	8	–
30...	8	9	–	8	8	8	8	8	7

[1] Rain. [2] Snow. [3] Cloudy.'

July	Hour of Observation									August	Hour of Observation								
	6	7	8	9	10-2	3	4	5	6		6	7	8	9	10-2	3	4	5	6
1...	–	5	–	6	–	–	6	6	–	1...	–	–	–	6	5	6	7	8	–
2...	–	8	–	–	4	–	–	–	–	2...	6	6	6	–	4	6	7	6	–
3...	–	8	7	–	6	–	–	–	4	3...	–	7	–	–	5	–	7	7	–
4...	–	6	–	7	5	–	–	–	–	4...	–	6	7	7	6	6	6	7	–
5...	8	8	–	–	5	–	7	–	6	5...	–	8	8	–	5	5	–	–	–
6...	–	8	8	–	7	–	–	8	8	6...	8	8	7	6	–	–	–	–	–
7...	–	8	7	–	–	–	–	–	–	7...	–	8	–	–	–	–	–	–	–
8...	8	8	7	–	7	–	–	8	8	8...	–	8	–	–	5	6	6	6	–
9...	8	9	–	–	6	–	–	–	–	9...	6	7	–	6	4	–	7	8	8
10...	–	–	–	–	–	–	–	–	–	10...	6	7	7	–	5	6	–	6	–
11...	–	8	6	–	5	–	7	8	6	11...	7	7	6	–	5	6	–	6	–
12...	8	9	8	–	5	6	7	7	7	12...	6	5	–	–	5	6	–	6	–
13...	8	8	7	7	6	–	–	8	7	13[4]...	6	–	–	–	–	–	–	4	–
14...	8	8	7	–	6	–	7	7	7	14[4]..	–	7	–	–	–	–	–	–	–
15...	–	7	6	5	4	4	6	7	–	15...	–	7	–	–	–	–	8	–	6
16...	–	5	4	4	–	–	–	–	–	16...	8	7	–	–	4	–	8	–	7
17...	–	–	–	–	–	–	–	–	–	17...	–	8	7	–	7	–	7	7	–
18...	8	8	8	7	6	–	–	–	–	18...	8	7	6	–	5	–	7	7	–
19...	6	8	7	7	6	–	–	–	–	19...	7	8	–	–	6	–	7	7	–
20...	7	8	7	6	4	–	6	8	7	20...	5	6	6	–	6	–	6	6	–
21...	–	–	–	–	–	–	–	–	–	21...	–	8	8	–	6	–	7	–	6
22...	–	7	–	6	5	–	6	7	–	22[1] ..	–	–	–	6	–	–	–	–	–
23...	–	7	–	6	5	–	6	7	–	23...	–	–	8	–	–	–	–	7	–
24...	–	7	–	–	–	–	–	–	–	24...	–	6	–	–	5	–	–	6	–
25...	–	6	5	4	4	4	5	–	–	25...	8	7	6	–	5	–	6	–	6
26...	–	–	6	5	4	5	6	7	–	26...	–	9	7	–	5	–	7	–	6
27...	–	6	5	4	4	5	6	7	–	27...	8	8	7	–	6	–	7	–	6
28...	7	8	7	–	4	–	–	4	–	28...	–	8	7	6	5	–	7	6	–
29...	–	7	6	–	4	–	8	7	–	29...	–	8	7	–	5	–	–	–	7
30...	–	8	7	6	6	–	–	–	–	30...	–	8	7	–	5	–	–	–	7
31...	–	–	–	–	–	–	–	–	–	31[3] ..	–	3	–	–	–	8	–	6	–

of the heating of the mountain then becomes apparent and the definition deteriorates. The disturbances at the Sun's limb under these conditions do not resemble those seen immediately after sunrise, but have a fluttering appearance, which we are accustomed to speak of as the "heating effect." In the late afternoon the seeing usually improves, but it is rarely very good at midday. This is not a rule without exceptions, however, as we have sometimes recorded nearly perfect definition during the hottest hours of the day.

Everyone who has noted the heated air above the surface of the ground will wonder, in considering the effect of such disturbances upon solar observations, whether these disturbances rise to a great height. A casual observation is sufficient to show that the disturb-

[1] Rain [2] Snow [3] Cloudy [4] Storm.

SEEING

September	Hour of Observation							October	Hour of Observation						
	7	8	9	10-2	3	4	5		7	8	9	10-2	3	4	5
1	8	6	–	5	–	7	7	1	7	6	–	5	–	7	6
2	7	8	6	5	–	7	6	2	8	7	–	5	–	–	–
3	8	6	6	5	–	–	7	3	8	7	6	5	–	7	–
4[3]	8	7	–	–	–	–	6	4[3]	–	–	–	–	–	6	–
5[3]	7	6	–	–	6	–	–	5[5]	8	–	–	–	–	–	–
6	8	7	6	5	–	–	7	6[9]	–	–	–	–	–	–	–
7	8	7	6	5	6	–	–	7[9]	–	–	–	–	–	–	–
8	8	7	–	5	–	7	7	8[9]	–	–	–	–	–	–	–
9	8	7	6	5	6	–	–	9[9]	–	–	–	–	–	–	–
10	8	–	6	5	–	7	6	10[9]	–	–	–	–	–	6	–
11	7	7	6	5	–	7	7	11[5]	7	6	–	–	–	–	–
12[3]	8	7	–	–	–	–	–	12	7	8	7	5	–	6	–
13	8	6	–	5	5	–	–	13	6	8	6	6	–	6	–
14	7	6	–	5	–	–	4	14	7	8	7	6	–	7	–
15	8	7	–	5	–	–	6	15	7	7	–	5	–	6	–
16	7	6	–	5	–	7	6	16	6	7	–	6	–	–	–
17[3]	7	6	–	–	–	–	–	17	6	6	–	5	–	6	–
18[5]	7	6	–	–	–	–	–	18	6	6	–	6	–	6	–
19	7	7	–	5	–	–	7	19	6	7	–	6	–	6	–
20[3]	7	6	–	–	–	–	–	20[6]	3	–	6	5	–	–	5
21[3]	–	–	–	–	–	–	–	21	6	6	–	5	–	6	–
22[9]	–	–	–	–	–	–	–	22	7	8	7	5	–	6	–
23[9]	–	–	–	–	–	–	–	23	7	7	7	5	–	6	–
24[9]	–	–	–	–	–	–	–	24	8	8	–	6	7	6	–
25[9]	–	–	–	–	–	–	–	25	7	8	–	6	6	8	–
26[9]	–	–	–	–	–	–	–	26	7	8	–	6	6	–	–
27	–	–	–	–	–	–	–	27	6	7	–	5	6	–	–
28	–	–	–	–	–	–	–	28	6	8	–	5	–	–	–
29	7	–	6	5	–	7	–	29	8	8	6	5	–	–	–
30	7	6	–	5	–	6	–	30[3]	–	–	5	5	8	7	–
								31	8	7	–	5	7	–	–

ance decreases rapidly in passing upward from the ground, but it is, of course, quite impossible to determine by means of the unaided eye the probable effect of this disturbance on telescopic observations. We have accordingly made many observations of the Sun with the 3¼-inch telescope supported in a pine tree at heights above the ground ranging from 20 to 80 feet. The results of these observations clearly indicate that a telescope employed in solar work should be mounted as high above the ground as circumstances warrant. At the lower elevations in the tree the advantage over positions still nearer to the ground was sometimes not appreciable; but at a height of 80 feet above the ground the improvement in definition was very distinct. Probably

3 Cloudy. 5 Fog. 6 High wind. 9 Fog and rain.

SEEING

November	Hour of Observation							December	Hour of Observation						
	7	8	9	10-2	3	4	5		7	8	9	10-2	3	4	5
1[3]	–	–	–	6	6	6	–	1[5]	–	–	–	–	–	–	–
2[3]	5	–	–	–	6	–	–	2[8]	–	–	–	–	–	–	–
3	6	6	–	5	6	–	–	3[7]	6	–	–	–	–	–	–
4[3]	8	7	–	5	–	–	–	4[3]	–	–	–	–	–	–	–
5	7	7	–	5	–	–	–	5[8]	–	5	–	–	–	–	–
6	–	8	7	5	6	–	–	6	5	6	–	5	5	–	–
7	7	7	–	6	6	–	–	7	–	6	–	6	6	–	–
8	7	–	–	5	6	–	–	8	–	6	–	5	6	–	–
9	6	–	–	5	6	–	–	9	–	7	–	6	6	–	–
10	5	6	–	5	5	–	–	10	–	–	–	–	–	–	–
11	6	8	–	5	5	–	–	11	–	–	–	–	–	–	–
12[3]	–	–	–	–	–	–	–	12	–	–	–	–	–	–	–
13[3]	–	–	–	–	6	–	–	13	–	6	6	5	6	–	–
14	6	6	–	5	–	–	–	14	–	7	–	5	6	5	–
15	–	–	–	–	6	–	–	15	–	6	–	5	5	–	–
16	7	7	–	5	6	–	–	16	–	8	7	6	6	–	–
17	8	7	–	6	6	–	–	17	–	8	–	6	6	–	–
18	7	7	–	5	6	–	–	18	–	8	–	6	7	–	–
19	7	8	–	6	–	–	–	19	–	8	7	5	6	–	–
20	–	7	6	6	6	–	–	20[3]	–	7	–	–	–	–	–
21	7	7	–	6	–	–	–	21[3]	–	–	–	–	–	–	–
22	7	8	–	6	7	–	–	22[9]	–	–	–	–	–	–	–
23	7	7	–	6	7	–	–	23[9]	–	–	–	–	–	–	–
24	–	–	–	–	–	–	–	24[9]	–	–	–	–	–	–	–
25	–	–	–	–	–	–	–	25	–	–	6	–	6	–	–
26	6	7	–	5	–	–	–	26	–	–	–	–	–	–	–
27[3]	–	–	–	5	6	–	–	27	–	–	–	–	–	–	–
28	7	7	–	5	7	–	–	28	–	7	6	5	6	–	–
29	7	7	7	6	5	–	–	29	–	8	7	6	6	–	–
30[3]	–	6	–	–	–	–	–	30[3]	–	–	7	–	–	–	–
								31[9]	–	–	–	–	–	–	–

this is one of the reasons why the solar definition with the 40-inch Yerkes telescope averages considerably better than we expected it would, for with this telescope the object-glass is over 70 feet above the ground.

OBSERVATIONS WITH THE FIFTEEN-INCH CŒLOSTAT TELESCOPE

In March 1904 a cœlostat of 15 inches aperture was sent to Mount Wilson from the Yerkes Observatory. This instrument had previously been employed by Professors Barnard and Ritchey, of the Yerkes Observatory party, at the solar eclipse of May 28, 1900, in Wadesboro, N. C., and by Professor Barnard at the Sumatra eclipse

3 Cloudy. 5 Fog. 7 Partly cloudy. 8 Cold and windy. 9 Fog and rain.

in 1901. As used at Mount Wilson, it is supplied with a second plane mirror, mounted south of the cœlostat, and arranged to slide on a north-and-south track in such a way as to receive the solar rays corresponding to any declination of the Sun.

The rays are reflected from this mirror toward the north to a 6-inch photographic objective of 61½ feet focal length, mounted on the extension of the stone pier just above the cœlostat. After passing through this lens the rays traverse a long tube built of wooden framework and covered with paper. The solar image is formed within a small house which terminates this tube at its north end. In the house a photographic plate-holder is mounted, in conjunction with a slide containing a narrow slit, which can be shot at high speed across the solar image by means of a spring. In this way the very short exposure required for direct photography of the Sun can be obtained.

One of the chief points of interest connected with this instrument is the effect of the heating of the air within the tube upon the definition of the solar image. In the first experiments with this apparatus, the skeleton tube was covered on all sides with tar-paper, just as it had been used in the eclipse work. Above the tube, and separated from it by a considerable air-space, was a canvas fly for the purpose of shielding the tube from the direct rays of the Sun. It was found that in the early morning, before the tube had become heated, the definition of the solar image was excellent. In a short time, however, heated air within the tube completely spoiled the definition, and the Sun's image became so blurred and indistinct that no observations of value could be made with it. These circumstances led us to question what the effect would be if no tube were employed. The 6-inch lens was therefore mounted in such a position as to throw the beam horizontally through the air toward the north, outside of the tube and over that portion of the ground which was in shadow. The image observed under these circumstances was found to be much better defined than that seen through the heated air of the tube. We accordingly decided to try the experiment of taking off all of the paper on the two sides which formed the upper half of the tube. It also seemed advisable to stretch the canvas fly at a much greater distance from the tube and to provide means of exit at the top for any heated air which might be found under the fly. As soon as the tube and fly had been re-arranged

in this manner, a great improvement was immediately noticed. The definition of the image became much better and the deterioration observed in the previous instance was no longer seen. The air in the tube remained cool, whereas before it had become greatly heated.

These experiments would seem to throw some light on the question of designing suitable tubes and shelters for telescopes used in a horizontal, or nearly horizontal, position. It seems likely that if the cœlostat and the instruments used with it could be mounted on piers at a height of 70 feet or more above the ground, it would be unnecessary to use any tube, particularly if the ground below the path of the beam were shielded from the Sun by a light canvas cover, stretched at a height of several feet above the surface and suitably ventilated. Of course, the practical difficulties in such a construction are very considerable, on account of the great cost and the lack of stability of high piers. For the Snow telescope it therefore seemed advisable to design a special form of house, in the hope of securing good definition with a solar beam at a moderate height above the ground. Experiments made with the 15-inch cœlostat seem to show that this latter instrument is too near the ground for the best results, although it gives excellent definition in the early morning, before the heating of the soil is very great.

The design of the house now under construction for the Snow telescope will be described in a subsequent report. It may be said here, however, that it consists of a skeleton frame of light steel construction, provided with a ventilated roof. The floor is to be of canvas, tightly stretched at a height of one foot above the ground and permitting a free circulation of air below. The inner walls of the house (which is 10 feet wide at its narrowest point) are to be of light canvas, so arranged that they can be raised or lowered at will. The outer walls of the house are to be covered by canvas louvres, so arranged as to shield the entire house from the direct rays of the Sun, and permitting a free circulation of air. The stone pier, 27 feet high, on which the cœlostat will stand, is also to be shielded from the Sun by canvas louvres. The ground surrounding the instrument is fairly well covered with bushes, and the few bare spots can be covered with stretched canvas, if necessary.

Spectroscopic observations.—The spectroscope used with the

cœlostat telescope is of the Littrow form: a single lens, of 4 inches aperture and 18 feet focal length, serves at once as collimator and camera lens. After passing through the slit, which is mounted in the focal plane of the photographic objective employed with the cœlostat, the rays pass to the 4-inch objective, by which they are rendered parallel. They then meet the 4-inch Rowland plane grating, having 14,438 lines to the inch, from which they are returned through the 4-inch objective. The image of the spectrum is formed on a photographic plate, mounted in the focal plane and a little to one side of the slit. This apparatus is giving excellent definition, surpassing that of any spectroscope employed at the Yerkes Observatory.

The character of the results obtained with this spectroscope, and its convenience of manipulation, illustrate one of the arguments in favor of fixed telescopes of the cœlostat type, as contrasted with moving equatorial telescopes. At the Yerkes Observatory it has never been possible to attach a sufficiently long and powerful spectroscope to the moving tube of the 40-inch refractor. Such a spectroscope must be mounted in a fixed position on substantial piers, and the telescope must be so constructed as to permit a sharp and well-defined image of the Sun to be maintained in a fixed position on the slit. This can readily be accomplished with the aid of a cœlostat, provided only that the difficulties peculiar to this type of telescope can be overcome. From the experiments so far made, we believe that the difficulties can be surmounted and that the fixed telescope is certain to become an instrument of great importance in the future.

CONCLUSION

From the observations given in this paper, it appears that Mount Wilson meets in a very remarkable degree the requirements of a site for a solar observatory. Indeed, I know of no other site that compares at all favorably with it. If a large solar observatory were established there, it might be expected to yield many important results, not to be obtained under less favorable conditions.

THE SOLAR OBSERVATORY OF THE CARNEGIE INSTITUTION OF WASHINGTON

By GEORGE E. HALE

In a report entitled "A Study of the Conditions for Solar Research at Mt. Wilson, California"[1] I have outlined the circumstances that have resulted in the establishment of a Solar Observatory on Mount Wilson[2] by the Carnegie Institution of Washington. At the recent annual meeting of the board of trustees, a grant of $150,000 was authorized, for use during 1905. It is expected that the first equipment will cost about twice this sum, and that important additions will result in the future from the operation of a large and well-appointed instrument and optical shop.

In April 1904 a grant of $10,000 was made by the executive committee of the Carnegie Institution for the purpose of bringing the Snow telescope to Mount Wilson from the Yerkes Observatory. An expedition for solar research was accordingly organized under the joint auspices of the University of Chicago and the Carnegie Institution, with the understanding that the funds granted by the Carnegie Institution would be used for the construction of piers and buildings, and for other expenses incidental to the work, while the University of Chicago would furnish the instrumental equipment, and pay the salaries of some of the members of the party. Messrs. Ritchey, Ellerman, and Adams, of the staff of the Yerkes Observatory, were to be associated with me in the work. While the executive committee of the Carnegie Institution indicated its intention of supplying further funds, if possible, for use during 1905, it was not supposed in April that provision could be made at present for the establishment of a large and fully equipped solar observatory. Nevertheless, it was agreed with the University of Chicago that if at any

[1] See *Contributions from the Solar Observatory of the Carnegie Institution* No. 1; *Astrophysical Journal*, March 1905.

[2] The approximate geographical position of the Solar Observatory, as given (by triangulation) by the U. S. Coast and Geodetic Survey, is as follows:

Latitude, 34° 13′ 26″
Longitude, 118° 3′ 40″.

time the Carnegie Institution should decide to establish a solar observatory of its own, such an observatory would take the place of the Mount Wilson Station of the Yerkes Observatory, and the work of the Station would be continued under the sole auspices of the Carnegie Institution.

AIM OF THE SOLAR OBSERVATORY

It has been my privilege to outline the plan of research and to determine the equipment of two other observatories. The Kenwood Observatory (subsequently merged with the Yerkes Observatory) had for its prime purpose the development of the spectroheliograph, and its use in solar research. The equipment was consequently designed with this purpose in view. The Yerkes Observatory had its origin in the gift of a 40-inch refracting telescope to the University of Chicago by Mr. Charles T. Yerkes. In designing the Observatory building, and in preparing a plan of research, I felt that the obligation of securing the greatest possible return from this powerful telescope must be a paramount consideration. In the nature of the case, a thoroughly homogeneous scheme of investigation could hardly be adopted for the Observatory under these circumstances, since the lines of work for which the 40-inch telescope is peculiarly fitted are very diverse in character.[2] The various applications of the Yerkes telescope in micrometric observations by Professors Burnham and Barnard; in stellar spectroscopy by Professor Frost and Mr. Adams, and by Mr. Ellerman and myself; in lunar, nebular, and stellar photography by Professor Ritchey; in the photographic study of stellar parallaxes by Dr. Schlesinger; in stellar photometry by Mr. Parkhurst; and in solar research with the spectroheliograph by Mr. Ellerman, Mr. Fox, and myself, will suffice to indicate that a serious attempt has been made at the Yerkes Observatory to realize the full possibilities of this magnificent instrument. But while recognizing the special demands of the 40-inch telescope, I have constantly kept in mind the development of other departments of the Observatory's work. Without enumerating these,[1] I shall confine my remarks to a line of effort which is of importance in the present connection,

[2] See "The Aim of the Yerkes Observatory," an address delivered at the formal inauguration of the work in 1897. *Astrophysical Journal*, **6**, 310, 1897.

[1] The results accomplished are epitomized in the *Reports of the Director*.

since it has defined the chief elements in the plan of research of the new Solar Observatory.

Both in the Kenwood and the Yerkes Observatories the instrument shop was regarded as of great importance, since it alone rendered possible the construction and frequent improvement of instruments of new type or special design. The Rumford spectroheliograph, the Bruce spectrograph, the two-foot reflecting telescope, and the Snow telescope are among the products of this shop. The operations of the shop were not confined to the construction of the mechanical parts of instruments; provision was also made for optical work on a large scale, under the direction of Professor G. W. Ritchey, who also succeeded Professor F. L. O. Wadsworth in the direction of the mechanical work.

In 1896, recognizing the great possibilities of the reflecting telescope for astrophysical research,[1] I engaged Professor Ritchey for the purpose of constructing a mirror of five feet aperture. An account of the methods employed in the grinding of this mirror has recently been given by Professor Ritchey.[2] My father's hope that he might be able to provide a suitable mounting for the five-foot mirror was frustrated by his death in 1898. At that time the fine grinding of the spherical surface had been completed, and the demands of other optical work rendered it advisable to discontinue further operations until funds for a mounting could be obtained. Many attempts were made to secure these funds, but they all proved ineffectual. Meanwhile, the success achieved by Keeler and Perrine with the three-foot Crossley reflector, and the remarkable results obtained by Ritchey with the two-foot reflector of the Yerkes Observatory, directed renewed attention to the possibilities of reflecting telescopes. It soon became clear that a five-foot mirror, if properly mounted, would give results entirely beyond the reach of existing instruments. The committee of the Carnegie Institution on the projects for southern and solar observatories accordingly felt that such a telescope should be included in the Solar Observatory equipment. The figuring and mounting of the five-foot mirror will therefore be undertaken as soon as possible.

[1] "On the Comparative Value of Reflecting and Refracting Telescopes for Astrophysical Investigations," *Astrophysical Journal*, **5**, 119, 1897.

[2] *Smithsonian Contributions to Knowledge*, Vol. XXXIV.

It is a fortunate circumstance that the construction and use of a great reflecting telescope is a logical element in the general plan of research laid down for the Solar Observatory. In *Year Book* No. 2,[1] of the Carnegie Institution may be found a report on this subject, prepared at the request of Professors Boss and Campbell, my colleagues on the committee, and improved in many particulars as the result of their criticisms. The prime object of the Solar Observatory is to apply new instruments and methods of research in a study of the physical elements of the problem of stellar evolution. Since the Sun is the only star near enough the Earth to permit its phenomena to be studied in detail, special attention will be devoted to solar physics. It is hoped that the knowledge of solar phenomena thus gained will assist to explain certain stellar phenomena. Conversely, the knowledge of nebular and stellar conditions to be obtained through spectroscopic and photographic investigations with the five-foot reflector should throw light on the past and future condition of the Sun. All of the principal researches will thus be made to converge on the problem of stellar development. The name "Solar Observatory" is regarded as appropriate, since the spectroscopic study of stars and nebulæ, to be carried on in connection with the solar work, are essential elements in any attempt to determine the mode of origin, the development, and the decay of the Sun as a typical star.

How, then, shall we attack in an effective manner the complex problem of stellar evolution? It goes without saying that I can offer no general answer to this question; I can only point out the three principal lines of attack which we hope to pursue at the Solar Observatory. These involve:

1. The more complete realization of laboratory conditions in astrophysical research, through the employment of fixed telescopes of the cœlostat type, and through the adoption of a *coudé* mounting for the five-foot reflector. This should permit: (*a*) the use of mirrors or objectives of great focal length, thus providing a large image of the Sun for study with spectroscopes and spectroheliographs; (*b*) the use of long focus grating spectroscopes, mounted in a fixed position in constant temperature laboratories, for the photography of stellar spectra requiring very long exposures; (*c*) the use of various labora-

[1] Page 49.

PLATE VIII.

LOWER PART OF MOUNT WILSON TRAIL, SHOWING PACK ANIMALS LOADED WITH LUMBER

tory instruments, such as the radiometer, which cannot be employed in conjunction with moving telescopes.

2. The development of the spectroheliograph in the various directions suggested by recent work at the Yerkes Observatory, including the photography of the entire solar disk with dark lines of hydrogen, iron, and other elements; further application of the method of photographing sections of flocculi corresponding to different levels; special studies of sun-spots, etc.; and daily routine records of calcium and hydrogen flocculi and prominences.

3. The construction of a five-foot equatorial reflector, with *coudê* mounting, and its use in the photography of nebulæ, the study of stellar and nebular spectra, and the measurement of the heat radiation of the brighter stars.

It was originally intended that a prolonged series of determinations of the solar constant, extending over at least one sun-spot period, should be made an important feature of the Observatory's work. The plans outlined in *Year Book* No. 2 accordingly included an equipment at Mount Wilson for this purpose, and suggested, in harmony with Dr. Langley's view, that provision be made for two additional stations, one near the summit of a high mountain, at an elevation of about 12,000 feet, the other at a much lower level on the same mountain. The principal purpose of these two stations was to measure the atmospheric absorption, in order to eliminate it from the solar constant determinations. The recent developments of Dr. Langley's researches at Washington have led Mr. Abbot, who is associated with Dr. Langley in the work, to the conclusion that entirely satisfactory results can be obtained there by the method employed. The poor atmospheric conditions with which the Washington observers have so successfully contended, and the disturbances arising from ground tremors in the heart of a large city, would be largely eliminated at Mount Wilson. For this reason it seems probable that results of higher precision could be obtained at this site. I have accordingly offered Dr. Langley facilities for pursuing the investigation at Mount Wilson, which I trust he may find it possible to accept.

In addition to the above-mentioned observations, provision will be made at Mount Wilson for various laboratory investigations necessary in conjunction with solar research. In view of the impor-

tance of securing a complete record of solar phenomena when magnetic storms are in progress, suitable magnetic apparatus, recommended by Dr. L. A. Bauer, in charge of the Department of Terrestrial Magnetism of the Carnegie Institution, will be installed at a sufficient distance from the electrical machinery.

TRANSPORTATION OF MATERIAL

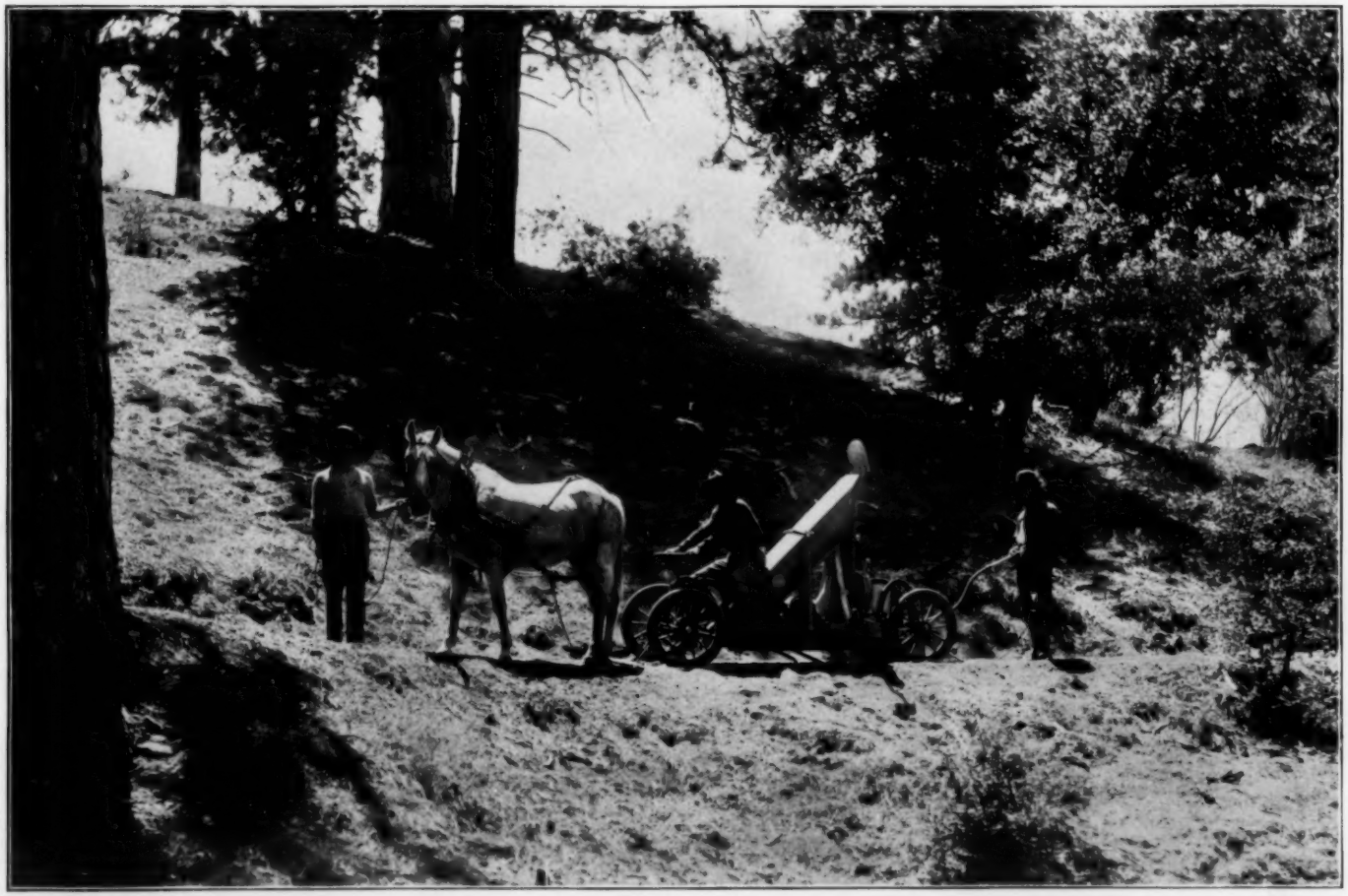
The first problem that confronts one in undertaking the construction of buildings and the erection of instruments on Mount Wilson is that of transportation over the trail from the valley. Two trails are available—the "Old Trail," from Sierra Madre, and the "New Trail," from the foot of Eaton Cañon, six and one-half miles from Pasadena. The New Trail, which is much the better of the two, is about nine miles long. At its narrowest points it is little over two feet in width, and in some of these places it had to be widened before the transportation of the heavy parts of the Snow telescope could be attempted. For ordinary packing with "burros" (donkeys) or mules the trail is well adapted. The loads brought up in this way range from 80 to 225 pounds per animal, and the charges from $1 to $1.35 per hundred pounds. On account of the expense of transportation over the trail, the best cement costs on the mountain more than twice as much as in the valley.

With a single exception, all parts of the 15-inch cœlostat, which was erected on Mount Wilson in April 1904,[1] were brought up on animals. The equatorial head of this instrument, which weighs about four hundred pounds, is too heavy to be carried in this way. A carriage was accordingly improvised for it from a two-wheel truck, such as is used by the railway companies for trunks. This served the purpose fairly well, though two days were required for the trip up the mountain. It was evident that a different arrangement would be required for heavier castings.

After provision had been made for the use of the Snow telescope on Mount Wilson, the carriage shown in Plate IX was designed. The running gear consists of four automobile wheels, 28 inches (71 cm) in diameter, with 2½ inch (6.3 cm) rubber tires. The distance between the

[1] See *Contributions from the Solar Observatory*, No. 1, *Astrophysical Journal*, March 1905.

PLATE IX



TRUCK FOR HAULING HEAVY INSTRUMENTS ON MOUNT WILSON TRAIL

wheels was limited by the width of the trail to 24 inches (61 cm). The bed of the truck is hung by wrought-iron yokes from the running gear, the lower surface of the bed being at a height of 6 inches (15 cm) above the ground. Steering gear, of the type used on automobiles, is provided for both pairs of wheels. A man riding on the load steers the forward wheels with a hand wheel, while the rear wheels are steered with a tiller by a man walking behind the carriage. A single large horse pulls a load of a thousand pounds on this carriage without difficulty. With two horses, used in relays, the trip from the lower end of the trail to the summit and return (a total distance of about nineteen miles) is completed with such a load in less than two days (about fifteen hours on the trail). With loads not exceeding 700 pounds the round trip is completed in a single day. Up to the present time the truck has made fifty round trips, carrying all the mirrors, lenses, and heavy castings of the Snow and Bruce telescopes, the parts of a fifteen horse-power gas engine, and other heavy machines, as well as the four-inch pipe columns (some of them twelve feet long) used in constructing the steel skeleton of the telescope house (Plate XI). The lighter angle-iron and other parts of the telescope house were brought up on burros. The total weight of material carried over the trail for the present work amounts so far to about 175 tons.

As the steering and control of the carriage on the narrow mountain trail is a difficult and dangerous task, special mention should be made of the excellent work of Mr. C. O. Sparks, who has been in charge of the carriage on all of its trips. It is to the credit of Mr. Sparks that nothing has been lost or injured during transportation.

Before the heavy castings (some of them weighing as much as five tons) required for the mounting of the five-foot reflector can be taken up Mount Wilson, the trail must be widened or some other mode of transportation provided.

THE SNOW TELESCOPE

As no description of this instrument has been published, the present brief account may be prefaced by a statement regarding the construction of the telescope.

In designing the Yerkes Observatory in 1894, I provided a large

heliostat room, 12 feet (3.66 m) wide and 104 feet (31.7 m) in length.[1] A small heliostat loaned by Professor Keeler was used in this room in 1897, and it was intended to mount permanently there, mainly for spectroscopic work, a combined heliostat and cœlostat designed by Professor Wadsworth.[2] When employed as a cœlostat, a second fixed mirorr was to be used with the instrument, so as to give the desired direction to the reflected beam.[2] Some of the patterns for this instrument were made, but the pressure of other work made it necessary to postpone the construction for some time.

In 1900, after Professor Ritchey had succeeded Professor Wadsworth as superintendent of instrument construction, a cœlostat with mirror of 15 inches (38 cm) aperture was made, from Professor Ritchey's designs, for the total solar eclipse of that year. This gave such satisfactory results that the plan of constructing a large cœlostat was again taken up. Unfortunately, however, no funds were available for this purpose. In 1901, during a visit to the Observatory of Professor Cross, chairman of the Rumford Committee, I showed him the details of the instrument, as worked out by Professor Ritchey. The design called for a cœlostat of 30 inches (76 cm) aperture, with second plane mirror of 24 inches (61 cm) aperture, the latter mounted so as to slide northeast and southwest on rails lying east of the cœlostat. The concave mirror, to which the light was reflected from the second plane mirror, had a focal length of 61 feet, and a second concave mirror, of 165 feet (50.3 m) focal length, was also to be used. For this reason the heliostat room, 104 feet (31.7 m) in length, was not long enough for our purpose, and the position of its axis, in the meridian, involved loss of light. It was accordingly necessary to erect a long wooden building, on the ground south of the Yerkes Observatory.

At the kind suggestion of Professor Cross, a grant of $500 was made by the Rumford Committee in aid of an investigation to be undertaken with this telescope. Subsequently, through the kindness of Professor Pickering, chairman of the Draper Committee, two other grants, of $500 each, became available. With these funds,

[1] See "The Yerkes Observatory of the University of Chicago," Part II, *Astrophysical Journal*, **5**, 260, 1897.

[2] *Ibid.*, p. 261.

helped out by small amounts obtained from other sources, the work was begun.

An account will be published later of this cœlostat and its accessory apparatus. The long wooden house on the Observatory grounds which contained it was destroyed by fire on December 22, 1902, through the breaking down of the insulation of a high-voltage electric transmission line, which supplied the spark used for a comparison spectrum. A 24–inch plane mirror and some of the mirror supports were saved, but most of the apparatus was completely destroyed or rendered useless.

Confident that the necessary funds could be obtained from some source, I decided to construct at once a new and better instrument, and to provide for it a more suitable house. Two important changes were made in the design. In the tests of the telescope, made by Mr. Adams and myself, the definition was poor, both in the case of the Sun and the stars. I attributed this in part to the fact that the cœlostat was mounted on a pier, the surface of which was only a few inches from the ground. This led me to observe distant objects at various heights above the ground with the naked eve, with field glasses and small telescopes, and finally with the 12-inch refractor, which stands on a pier about 40 feet (12.2 m) high. I soon reached the conclusion that the cœlostat must be mounted as far as possible above the ground, and that a site shaded by low trees or bushes would be much better than unshaded soil.

A gift of $10,000 from Miss Helen Snow, of Chicago, in memory of her father, the late George W. Snow, provided sufficient funds to complete the telescope and to instal it in a suitable house. The cœlostat was mounted on a brick pier, at a height of 15 feet (4.57 m) above the ground. In Professor Ritchey's design of the previous instrument the rays were reflected in a northeasterly direction from the cœlostat mirror to a second plane mirror, which sent them toward the southwest to one or the other of the concave mirrors. In designing the Snow telescope, a new arrangement of the second mirror was adopted by Professor Ritchey, at the suggestion of Mr. C. G. Abbot. As Plate X indicates, the light is reflected upward and to the south from the cœlostat mirror to a second plane mirror, mounted in a fork at the upper extremity of an iron column, on a carriage

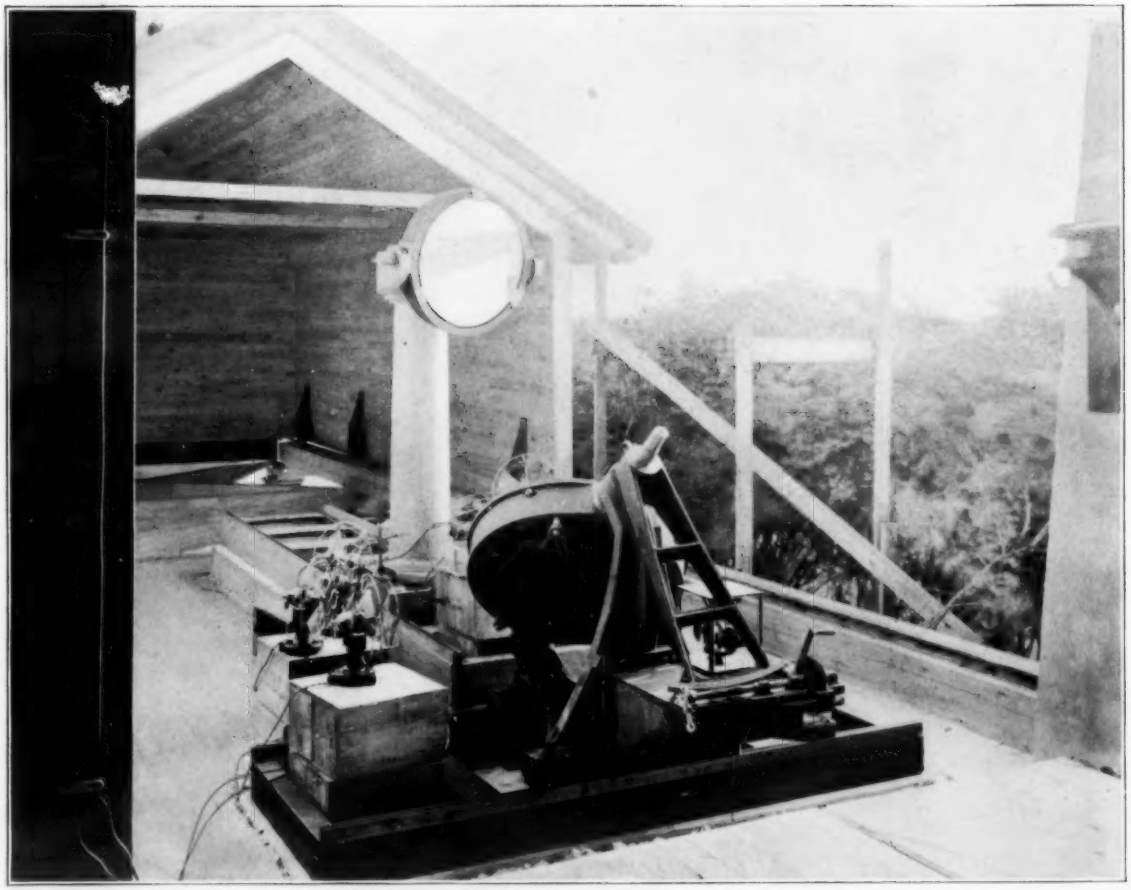
which can be moved along heavy iron rails. The position of this carriage on the rails depends upon the declination of the observed object: with a low Sun the second mirror stands close to the cœlostat, but with a high Sun it must be moved away in order to intercept the reflected beam. The cœlostat itself may be moved east or west on its own rails, so that a low object near the meridian may not be hidden by the second mirror or its support.

With the exception of the solar and stellar spectroscopes, for which suitable gratings could not be obtained, the Snow telescope was practically completed in the autumn of 1903. On October 3 of that year it was formally presented to the University of Chicago by Miss Snow, in the presence of a number of guests. Dr. George S. Isham, on behalf of Miss Snow, made the presentation address. The address of acceptance was made by Dean R. S. Salisbury, of the Ogden Graduate School of Science. The manner of using the telescope was afterward demonstrated.

The tests of the telescope made at this time seemed to indicate a decided improvement in definition, which I attributed to the greater elevation of the cœlostat. The Sun's image was frequently well defined, in spite of the change of focus due to the heating of the mirrors. Of this change more will be said later. At present I wish to refer especially to the definition as affected by the design of the telescope house.

The parallel beam from the second mirror was reflected due north through a spectroscopic laboratory into a long, narrow room, at the end of which the concave mirror stood on a massive brick pier. After striking the mirror, the beam was reflected back, so as to form an image of the Sun or a star in the spectroscopic laboratory a short distance from the axis of the parallel beam. The walls and floor of the house are of wood, and the question arises whether their radiation may not heat the air in the house, and thereby affect the definition. In general, the temperature of the air within such a house must differ in some degree from that of the air outside. Hence, some effect on the definition might be expected. The warm air rising about the cœlostat, due in part to the heating of the wooden walls which surround the pier, may also cause some disturbance of the image.

PLATE X



THE SNOW TELESCOPE WHEN MOUNTED AT THE YERKES OBSERVATORY

Rayleigh has shown that in a telescope tube only 12 cm long, a stratum of air in the upper part of the tube, occupying only a moderate fraction of the entire volume, would produce a sensible effect on the definition if heated 1° C.[1] In a tube 60 feet (18.3 m) long, through which the beam passes twice, the difference in temperature of a stratum, required to produce a similar effect, would be only about one three-hundredth of a degree. The assumed retardation is one-quarter of a wave, and the change of temperature from one side of the beam to the other is supposed not to be uniform.

To the practical observer such a result may seem to have little meaning. I have repeatedly seen the solar image beautifully defined with the 40-inch (102 cm) Yerkes refractor, when the air within the tube had become greatly heated—and certainly not uniformly so—after hours of continuous observation. Indeed, it is difficult to understand how such excellent definition can be obtained under these conditions. For in the optical testing-room Rayleigh's conclusions are easily verified. The great difficulty of securing a satisfactory test of a mirror by the Foucault test is well known; with a focal length as great as 145 feet (44.2 m) our opticians have waited for weeks to obtain a satisfactory test, even in the quiet air of the long testing-room in the basement of the Yerkes Observatory. The trouble resulting from stratification of the air, and the disturbance caused by the proximity to the beam of a person's hand, are familiar to all opticians. With these difficulties in mind, the problem of obtaining really good images of the Sun appears very serious. Yet the fact remains that good images are sometimes obtained. The great height of the 40-inch objective above the ground is probably an important advantage of this telescope, though the radiation of the dome on each side of the shutter-opening must produce some disturbance. The mere heating of the cell of the objective by the Sun would seem to be a sufficient cause for serious disturbance of the definition. Of course it is extremely probable that with sufficiently good atmospheric conditions all of these heating effects are actually perceptible in some degree, and that if they could be eliminated the seeing would

[1] *Collected Papers*, Vol. I, p. 434.

be much better than it is now. A skeleton tube, with a simple device for shading the cell, would probaply be advantageous.[1]

During the tests of the Snow telescope at the Yerkes Observatory, Langley's plan of stirring the air along the path of the beam was tried. The beam was made to pass through a tube of thick building paper, supported on a light wooden frame of square section, about 36 inches (91 cm) square. Electric fans were mounted at openings cut in the walls of the tube. When running at high speed, they kept the air within the tube in constant motion. At times the image of the Sun was distinctly improved in definition soon after the fans were started; but in other cases no improvement whatever resulted. It nevertheless seemed probable that a modified method of stirring the air might advantageously be employed for the Snow telescope house on Mount Wilson, and a tentative design was prepared. But further tests, made at my request by Professor Ritchey, indicated that we could not hope for satisfactory results without much more experimenting than we could afford to undertake. I accordingly abandoned this plan, and designed the cœlostat house described below.

CŒLOSTAT HOUSE ON MOUNT WILSON

In designing the new cœlostat house, I was influenced by two principal considerations:

1. The importance of placing the cœlostat as far as possible above the ground, which had been indicated by observations made with a telescope in a tree at elevations ranging from twenty to seventy feet.

2. The importance of constructing the house in such a way as to reduce to a minimum the heating and the radiation of the floor, walls, and ceiling, with the purpose of keeping the air within the house at the same temperature as the outer air.

In plan (Fig. 1), the building resembles the Snow telescope house at Williams Bay. The cœlostat stands on a carriage, which

[1] The case of the 40-inch telescope tube is doubtless hardly comparable with that of the Snow telescope house. The 40-inch tube is sealed by the objective at the upper end, and there is little mixture of the heated air of the tube with the cooler air outside. For this reason it would be interesting to close the end of the telescope house (near the cœlostat) with an objective, and try the definition under such conditions, i. e., without the concave mirror.

can be moved east or west along the rails, *a a*. On account of the configuration of the ground, which falls rapidly toward the north, it was necessary to make the long axis of the building run fifteen degrees east of north, instead of being exactly in the meridian. For the same reason this axis is not horizontal, but inclines downward five degrees toward the north. Without these modifications of the original plan, the height of the northern part of the building would have been very great, involving serious increase of expense. The rails *b b*, on which the carriage bearing the second mirror slides, are parallel to the optical axis.

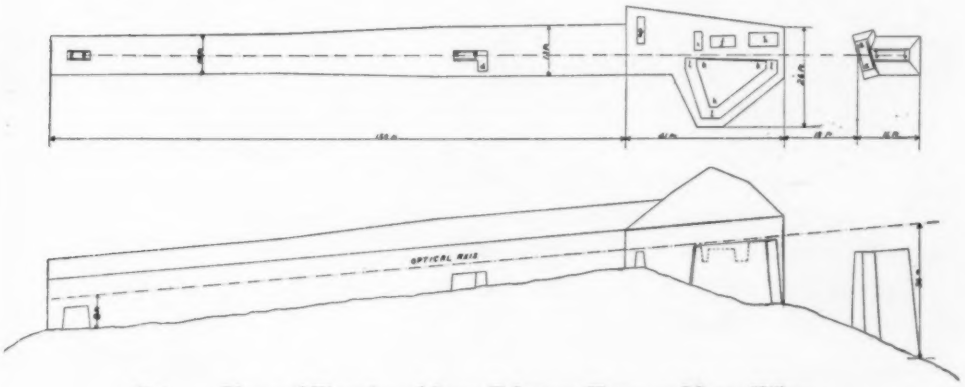


FIG. 1.—Plan and Elevation of Snow Telescope House on Mount Wilson.

Two concave mirrors, each of 24 inches (61 cm) aperture, are to be used. Of these, the mirror of 60 feet (18.3 m) focal length is mounted on its carriage so that it can be moved (for focusing), along the rails *c c*. The mirror of 145 feet (44.2 m) focal length is to be similarly mounted on the rails *e e*. When the long-focus mirror is to be used, the mirror of 60 feet focus is moved to one side, on the extension of its pier at *d*.

In designing the spectroscopic apparatus for the telescope, I have had the benefit of valuable suggestions from all members of the staff. The instruments are to be five in number, as follows:

1. A spectroheliograph with portrait lenses of 8 inches (20.3 cm) aperture, and 60 inches (152 cm) focal length, provided with four dense flint prisms. This instrument is to be carried on the pier *f*, where it will be floated in mercury (to reduce the friction of steel balls

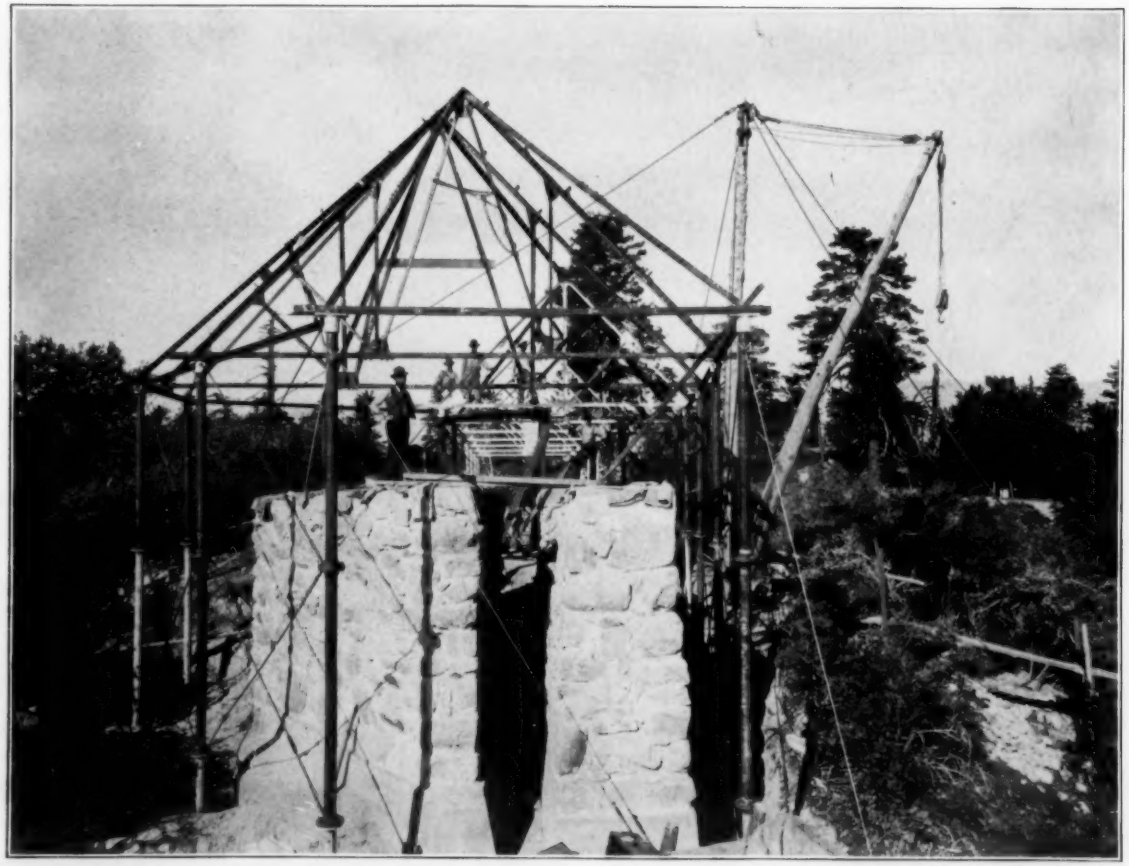
running in V rails), and moved as a whole across the 6.7 inch (17 cm) solar image given by the mirror of 60 feet focal length. The principal purpose of the instrument is to secure daily photographs of the entire solar disk with the calcium and hydrogen lines.

2. A spectroheliograph with lenses of 5 inches (12.7 cm) aperture, and 30 feet (9.14 m) focal length, provided with three light flint prisms of 50° angle. The first and second slits of this spectroheliograph are to stand on the pier *g*, while the collimator and camera lenses and the prism train will be carried by the pier *h*. The spectroheliograph will be fixed in position, and the 16-inch (41 cm) solar image given by the mirror of 145 feet focal length will be moved across the slit by a slow motion of rotation, about a vertical axis, of the 145 foot mirror. At the same time, the photographic plate will be moved synchronously across the second slit. The principal purpose of this instrument is to photograph zones about 4 inches (10 cm) wide of the large solar image, using the lines of iron and other elements which are too narrow to be employed with spectroheliographs of small dispersion. The instrument will also be employed with a plane grating, as a spectroscope for the study of the spectra of sun-spots, etc.

3. A Littrow spectrograph of 18 feet (5.49 m) focal length, with large plane grating. The single objective (on pier *h*), that serves for collimator and camera, will form an image of the spectrum just above the slit (on pier *j*). This spectrograph will be used with the 60-foot mirror, mainly for a study of the solar rotation and the spectra of sun-spots.

4. A concave grating stellar spectrograph, of about 15 feet (4.57 m) equivalent focal length, mounted on the massive pier *k k k* in the constant-temperature room *l l l*. A collimating lens of 5 inches (12.7 cm) aperture will be used with the grating, in order to avoid astigmatism. For the present, until a suitable concave grating can be obtained, a plane grating will be used with a camera lens of 5 inches aperture, and about 13 feet (3.96 m) focal length. This spectrograph is to be employed with the 60-foot mirror in an attempt to photograph, with high dispersion, the spectra of some of the brightest stars. The fixed position of the spectrograph on a massive stone pier, and the possibility of maintaining the grating at a constant temperature, should render very long exposures feasible.

PLATE XI



HOUSE FOR SNOW TELESCOPE ON MOUNT WILSON
Looking North from Cœlostat Pier

5. A prism spectrograph, with collimator lens of 1½ inches (3.8 cm) aperture and 45 inches (114.5 cm) focal length, dispersion of from one to four prisms, and camera lenses of various focal lengths, all of ultra-violet glass. The optical parts of the spectrograph will be mounted in such a way that they can be used on the large pier in the constant-temperature room, in conjunction with the slit of the concave grating spectrograph. The prism spectrograph will be used for special studies of stellar spectra, especially in the ultra-violet region.

It is to be understood that instruments 1, 2, and 3 are to be so supported, at different levels, that they will not interfere with one another, and will always be ready for use. The prism spectrograph, however, must be moved to one side when the concave grating spectrograph is to be employed.

The arrangement of the apparatus having thus been explained, let us consider more particularly the construction of the building. As Plates XI and XII show, the structure is of steel, as light as due regard for occasional high winds will permit. Steel guy ropes, anchored to large masses of concrete, afford the additional strength required in the heavy storms of winter. Since the parallel beam from the cœlostat to the concave mirror passes through a closed tube, it is not essential that this part of the building should stand high above the ground. Where the rays of the Sun fall upon the cœlostat itself, however, there can be no protection of the beam, and consequently it is desirable that the cœlostat should stand at a considerable elevation. After many tests of the seeing had been made at various points on the mountain, a site was finally selected which seemed to meet the required conditions. The cœlostat pier stands on a south slope, commanding a practically unobstructed horizon. At its south end this pier rises 29 feet (8.8 m) above the ground; hence, as the center of the second mirror is 74 inches (1.88 m) above the pier, the optical axis of the telescope is at this point about 35 feet (10.7 m) above the ground. At the north end of the pier the rising slope of the hill decreases this height to about 25 feet (7.6 m). When not in use, the cœlostat and second mirror are covered by a house on wheels, closed at both ends by double walls of heavy canvas. These may be opened, so that when the house is moved to the north, the cœlostat stands completely exposed. The movable shelter then

fits closely against the south wall of the spectroscopic laboratory, and thus forms a part of the tube through which the beam passes. When in this position the shelter has a canvas floor, so that the beam is completely protected from ascending currents after it leaves the north end of the cœlostat pier.

All parts of the building, including the movable shelter, the spectroscopic laboratory, and the long, narrow house extending north from the spectroscopic laboratory, have an inner wall and ceiling of canvas, and an outer wall composed of canvas louvres, very completety ventilated. The roof is also ventilated, by wooden louvres at the ridge, thoughout the entire length of the movable shelter and the north extension, and at the peak of the laboratory. Rain and snow are prevented from entering the roof louvres by means of canvas curtains, which can be raised or lowered at will. The house extending north from the laboratory has a floor of canvas, with an air-space below, through which the air may pass freely.

In traversing the spectroscopic laboratory, the beam necessarily passes very close to the wall of the constant-temperature room. To diminish the effect of radiation from this wall, a covering of sheet metal is arranged so that a constant current of air may be drawn between the sheet metal and the wall by means of an exhaust fan. Outer air is brought in from the west side of the laboratory, and no internal drafts are created, as the connections between the air-space and the supply and exhaust tubes are perfectly tight. It is hoped that any evil effects of radiation from the stone piers and the wooden floor of the spectroscopic laboratory can be eliminated by similar devices.

The louvres surrounding the cœlostat pier are intended to protect the pier from vibration caused by the wind, and from heating by the Sun. The steel structure does not touch the pier at any point, and is therefore made rigid enough to support itself in high winds.

The cœlostat, and the supports for the plane mirror and the 60-foot concave mirror, are now in place on the piers, but heavy storms have prevented the mirrors from being mounted. The concave grating stellar spectrograph is nearly ready to be set up, and work is well advanced on the smaller of the two spectroheliographs. The ultra-violet glass prisms and lenses for the stellar spectrograph have been

PLATE XII

HOUSE FOR SNOW TELESCOPE ON MOUNT WILSON
Showing Cœlostat Pier and Movable Shutter

completed by the Carl Zeiss Company, and orders have been placed for the optical parts of the 30-foot spectroheliograph and the Littrow spectrograph. Through the courtesy of the president and trustees of the University of Chicago, the Snow telescope and some of its accessories will be used by the Solar Observatory for some time. It will subsequently be replaced by a similar telescope constructed in our own instrument shop.

THE HOOKER EXPEDITION

As the result of a gift of $1,000, made by Mr. John D. Hooker, of Los Angeles, the Bruce photographic telescope of the Yerkes Observatory has been brought to Mount Wilson by Professor Barnard, for use during a period of several months, after which it will be returned to Williams Bay. A full description of this telescope has recently been published by Professor Barnard in the *Astrophysical Journal*.[1] The house built for the Bruce telescope on Mount Wilson has a sliding roof, which leaves the entire sky free when it is pushed back. Professor Barnard has already obtained some excellent photographs of *Orion* and other constellations. Their quality is such as to give promise of important results, as soon as the stormy weather of the rainy season abates sufficiently to permit long exposures to be given.

THE "MONASTERY"

In the original estimates for the Solar Observatory, made by the committee of the Carnegie Institution, $52,500 was set apart for the construction of dwelling houses on Mount Wilson for the families of the staff, and $51,000 for a large building, containing offices for all the members of the staff, and rooms for laboratories and instrument shops. In these particulars the report simply adopted the plan followed by the Lick and Yerkes Observatories. A residence of six months in a log cabin on Mount Wilson, under conditions which rendered necessary the greatest economy of expenditure, convinced me that a better use could be made of the Institution's funds. In the first place, it is by no means desirable to confine families, and especially children deserving every educational advantage, within the narrow limits of an isolated observatory colony. Furthermore,

[1] **21**, 35–48, 1905.

the work of an instrument shop, and much routine computing as well, can be done at much less expense and to better advantage in a town, where foundries and various sources of supply are at hand, and better workmen and computers can be employed. Finally, a great economy can be effected by using funds for instruments, machinery, and books—the tools of the investigator—that would otherwise be spent for mere buildings. In short, I believe the principle should be recognized that the mountain site is valuable for *observations*, and that most other classes of work can be better done elsewhere. These considerations strike one most forcibly in a place where the cost of building materials is doubled by transportation over the trail.

The isolation of most mountain sites, however, might seem to demand that the staff of such an observatory should be composed only of celibates, or that its members must be content to experience long periods of separation from their families. In this particular Mount Wilson is most fortunately situated. The city of Pasadena, which is hardly to be surpassed as a place of residence, lies at the very foot of the mountain, and can easily be reached in two and one-half hours. Los Angeles, with its large machine shops, foundries, and supply houses, is also near at hand. It is thus perfectly feasible to have the families of the observers live in Pasadena, where members of the staff can spend Sundays, and go on business at other times. We are following this plan, and find it is as satisfactory as could be expected under the circumstances.

I consider it very desirable that each member of an observatory staff, if engaged in work requiring concentrated attention, such as computing or measuring, should have a workroom of his own. The space occupied may be very small, but it should certainly be set apart for individual use. This requirement, together with the necessity of supplying living accommodations for the astronomers, determined the design of the "Monastery."

As shown in plan in Fig. 2, the building has two wings; one containing the dining-room, kitchen, pantries, two bedrooms, woodshed, etc.; the other, the bedrooms and offices of the astronomers, guest-room, bathroom, etc., opening on a long, narrow hall. Each astronomer has a small bedroom and an adjoining office. The

convenience of this arrangement, and the satisfaction it has given to all the members of the staff, indicate that a great saving in expense, without loss of efficiency, will be effected. The large room, with stone fireplace, which unites the two wings, serves as a general library. The "Monastery" stands at the extreme end of a narrow point with precipitous walls, and commands a fine view of the neighboring mountains, the San Gabriel Valley, the cities of Pasadena and Los Angeles, and the Pacific Ocean. It was designed, after our plans, by Messrs. Hunt & Gray, architects.

FIG. 2.—Plan of the "Monastery."

Through a recent gift from Mr. John D. Hooker, a small guest-house, containing two bedrooms and a living-room, will soon be erected near the Monastery.

POWER HOUSE AND REPAIR SHOP

A one-story building, 15×35 feet (4.57×10.67 m), situated between the Snow telescope and the "Monastery," is equipped as a small power-house and repair shop. It contains a 15 horse-power Witte gasoline engine; 7½ K. W. dynamo, giving either alternating or direct current; storage battery of thirty cells, small screw-cutting lathe; Rivett milling machine; sensitive drill; emery grinder; Oliver trimmer; forge and anvil; and a good assortment of small tools needed for repairs and for a certain amount of construction work.

Although our large instrument shop in Pasadena is prepared to undertake any class of work, I regard a small shop on the mountain as indispensable. The engine, dynamo, and storage battery furnish current for arc and spark discharges, and for temperature control required in spectroscopic work, power to run the spectroheliographs, exhaust fan, etc., and light for the offices and laboratories.

A line for transmitting electric power from the San Gabriel Valley will be installed later, since much more power will be required for the 5-foot reflector and other purposes. The present power plant was provided before it was known that a large Solar Observatory would be established by the Carnegie Institution.

It was originally intended to supply water to the various buildings from a well at Strain's Camp, about 325 feet (99 m) below the summit of Mount Wilson; but as the well yielded almost no water last autumn (after an unusually dry period), it is likely that a more reliable source will be chosen.

GENERAL LABORATORY

A small laboratory building, probably of fire-proof construction, will be erected near the Snow telescope in the spring. This will contain a large grating spectrograph, with various accessory apparatus, such as a Du Bois half-ring electromagnet for the Zeeman effect, an arc in pressure chamber, a transformer and condenser for studies of spark discharges, etc. The equipment will also include a Pulfrich stereocomparator, principally for the study of spectroheliograph plates; an Abbe spectrometer; an interferometer, for the measurement of absolute wave-lengths; measuring machines for spectra and for stellar photographs; globe for the measurement of heliographic positions, etc. In addition to the spectroscopic laboratory, the building will contain a small chemical laboratory, an enlarging room, photographic dark-rooms, rooms for the storage of negatives, etc. A visible recording variometer and magnetic storm detector will be established in a separate building, for use in connection with the solar observations.

PASADENA OFFICES AND LABORATORIES

With the invaluable assistance of the Pasadena Board of Trade, a piece of land, on Santa Barbara Street, 150 feet (45.7 m) front by

208 feet (61 m) deep, has been secured for the Pasadena offices and instrument shop. The building, which is now under construction, was designed by Professor Ritchey. It is 50×100 feet (15.2×30.5 m) in size, with an optical testing-room, 150 feet (45.7 m) long, extending 68 feet (20.7 m) beyond it in the rear . The walls are of brick, and the floor of cement. Pains will be taken to make the structure throughout as nearly fire-proof as the available funds will permit, since the optical and mechanical parts of instruments under construction will be very valuable.

The building will contain offices for Professor Ritchey and myself, and a stenographer; drafting-room, machine shop, instrument shop, pattern shop, lacquering-room, constant-temperature room, room for 5-foot (1.5 m) grinding machine, room for 40-inch (1 m) grinding machine, long optical testing-room, photographic dark-rooms, enlarging-room, etc. The equipment includes a No. 2 Brown & Sharpe Universal milling machine, 24×24-inch (61×61 cm) Gray planer, 20-inch (51 cm) Hendey-Norton engine lathe, 12-inch (30.5 cm) Hendey-Norton tool maker's lathe, No. 4 Rivett bench lathe, No. 2 Landis Universal grinding machine, drill press, pattern-maker's lathe, circular saw, band-saw, Oliver trimmer, automatic hack-saw, emery grinder, etc. The supply of small tools is very complete. The optical laboratory will contain all necessary machinery for grinding, polishing, and testing mirrors, with apertures as great as 5 feet (1.5 m), and focal lengths as great as 150 feet (45.7 m).

Most of the above machine tools are now in use at the instrument shop temporarily occupied in the Seward Building, between Colorado and Union Streets. At present, two draftsmen, one instrument-maker, three machinists, and two pattern-makers are at work there, under the direction of Professor Ritchey. No optical work can be done until the new shop is completed.

It is probable that offices for a staff of computers will ultimately be provided adjoining the instrument shop.

EXPERIMENTS WITH FUSED QUARTZ

As already stated, glass mirrors are subject to change of figure when exposed to the Sun's rays. At the independent suggestion of Dr. Billings and Dr. Elihu Thomson, experiments have been undertaken with the object of using fused quartz instead of glass for the

mirrors, since its coefficient of expansion is only about one-tenth as great. The work has been done by Professor Ritchey and Mr. Wingren, with the aid of a grant given last spring for this purpose by the Carnegie Institution. The quartz is easily fused in an electric furnace, but the fused mass is filled with fine bubbles, which increase in number and size as the temperature of the furnace is increased. An attempt is being made to eliminate the bubbles, in order to secure fused quartz for prisms and lenses. For mirrors, as Professor Ritchey suggests, blocks like those already obtained will probably serve very well, if the bubbles at one surface can be gotten out by remelting with the flame of an electric arc.

STAFF

The staff of the Solar Observatory is at present constituted as follows:

George E. Hale, Director.

G. W. Ritchey, Astronomer, and Superintendent of Instrument Construction.

Ferdinand Ellerman, Assistant Astronomer.

Walter S. Adams, Assistant Astronomer.

There is a post-office on Mount Wilson, about a mile from the Observatory, but the delivery of mail is so infrequent and irregular, that I conduct my correspondence from the Observatory Office in Pasadena, where letters for me should be addressed. Letters and printed matter for Professor Ritchey should be sent to the same address, but printed matter intended for me should be addressed to Mount Wilson, Cal., as my scientific library is at the Observatory. Printed matter for the Solar Observatory, and both letters and printed matter for Mr. Ellerman and Mr. Adams, should be addressed to Mount Wilson.

Books and papers, especially on astrophysical subjects, will be gladly received for the library of the Solar Observatory. Sets of observatory publications, which are greatly needed, may be forwarded from abroad, free of expense, through the International Bureau of Exchanges of the Smithsonian Institution, which has offices in the principal cities of Europe. It is hoped that some return for such contributions may ultimately be made in the form of our own publications.

MOUNT WILSON, CAL.,
February 8, 1905.

INTENSITY OF GRATING SPECTRA

By R. W. WOOD

Having had occasion recently to plan for the construction of a short-focus spectrograph of fairly large dispersion in the visible region, the question of gratings *versus* prisms came up. Plenty of data regarding prisms are to be found in Kayser's new treatise in spectroscopy, but little or nothing seems to have been published regarding gratings, the only statements made being rough guesses. It seemed worth while to make a study of the distribution of the light (monochromatic) in the different spectra of a typical grating.

The apparatus which my assistant Mr. Pfund arranged for making the measurements was very simple, and the whole thing was accomplished in half an hour. The grating selected was a fairly typical one, the central image rather dark and of a brownish color, indicating that no very marked selective action for certain colors was present, and the first-order spectrum on one side extremely bright. The measuring apparatus, or photometer, consisted of a pair of Nicol prisms (one mounted in a graduated circle), a small piece of silvered glass, and a bright and uniform sodium flame. The silvered glass can be made by dissolving the varnish from the back of a piece of modern mirror, and polishing with rouge. It is mounted vertically at an angle of 45° with the axes of the nicols, and covers the lower half of the field (Fig. 1). The soda flame is immediately behind the polarizing prism, and the grating stands to one side, as shown in the figure. By turning the grating the central, or any one of the lateral

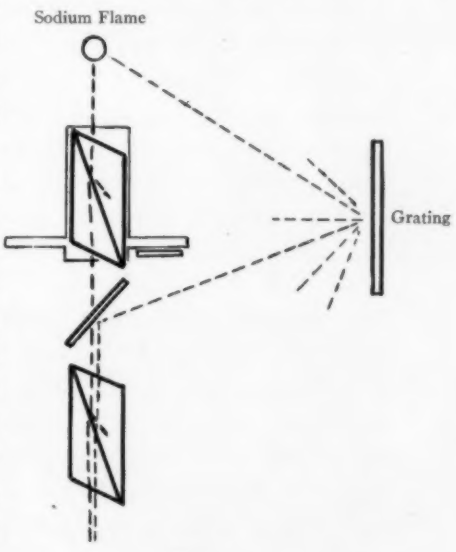


FIG. 1.

(spectral), images of the flame can be viewed in the silvered mirror immediately in contact with the image of the flame seen through the nicols, and by turning one of them the intensities can be accurately adjusted. We first set the graduated nicol in the zero position, and then turn the other nicol to the position of extinction. The intensity of the restored light for a given angle measured from this position is proportional to the square of the sine of the angle. The central image can be located easily by watching for the reflection of the flame in the unruled portion of the surface. The results obtained are recorded in the following table, eight spectral images being measured:

Fourth Spectrum	Third Spectrum	Second Spectrum	First Spectrum	Central Image	First Spectrum	Second Spectrum	Third Spectrum	Fourth Spectrum
0.073	0.057	0.20	0.31	0.16	0.98	0.096	0.032	0.01

The numbers given are the squares of the sines of the angles, and represent the intensities of the images as fractional parts of the light transmitted through the first nicol. The intensity of the first spectrum on the right is as great as the sum of all the others together with the central image (0.94), which amounts to saying that half of the total light reflected is found in one spectrum.

It is frequently stated that a nicol reduces the intensity of unpolarized light by one-half. The reduction is obviously greater than this on account of the reflections at the two oblique surfaces, and to a slight extent by the balsam film. In the present case the surfaces of the prism were slightly dull, and I doubt if the intensity of the transmitted light was much over 40 per cent. of the original intensity. Calling the intensity of the soda flame 100, we get the intensities of the spectra by multiplying 40 by the fractions given in the table. The sum of these intensities (eight spectra and central image) is 75.6, which agrees fairly well with Rubens' determination of the reflecting power of spectrum metal for yellow (70 per cent.). This indicates that the ruling of the surface interferes in no way with the *total* reflection, which is what might be expected. The interesting point is that half of the total light is found in one spectrum. If speculum reflects 70 per cent., this means that we have 35 per cent. of the light in the first-order spectrum, or about one-third of the original amount.

To determine whether or not this was the case, I arranged a photographic lens, soda flame, and the grating in such a way that the lens pictured the *direct* image of the flame and the first-order spectral image side by side on a photographic plate (Fig. 2). A ray filter of aurantia was placed in front of the plate, to eliminate the action of the blue and green rays from the Bunsen flame, which would be present in the direct image, and absent in the spectral image. The spectral image was exposed thirty seconds, and the direct ten, and on developing it was found that the images had almost exactly the same intensity, showing that no error of any considerable amount had been made in the photometric work. Of course, the same intensity distribution might not, and probably would not, be found

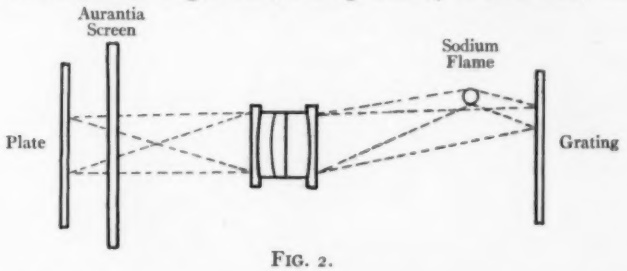


FIG. 2.

for other colors, but the results obtained with sodium light give a fair idea of what may be expected of an average grating.

The difficulty of ruling satisfactory gratings of very short focus (one meter or less), combined with the fact that only short lines can be ruled, makes it appear probable that for work of certain kinds better results can be expected with large plane gratings combined with achromatic lenses. Two flint prisms of 60° would give us about the same average dispersion, and the intensity would be a little more than double that given by the grating, since two prisms of this description transmit 75 per cent. of the light, according to Pickering's table given in Kayser's *Handbook*. The measurements which I have recorded were made merely for my own information, without any idea of publication. As they may prove of some interest to others, it has seemed worth while to put them on record. I have, of course, said nothing about resolving powers in considering the two types of dispersion pieces.

JOHNS HOPKINS UNIVERSITY,
January 8, 1905.

THE VARIABLE RADIAL VELOCITY OF *SIRIUS*

BY W. W. CAMPBELL

The increasing number of binary stars, of relatively short period, whose internal motions can be observed by both micrometric and spectrographic methods, requires that radial-velocity observers should deal somewhat extensively with double-star orbits. This subject, naturally simple, is unfortunately complicated by the use of a great many heterogeneous systems and nomenclatures to define the orbital elements. Before proceeding to discuss one of the stars of the class described, it seemed desirable to formulate, if possible, a simple system and nomenclature of double-star elements which should meet all the requirements in that department of astronomy, which would readily adapt themselves to the evidence of the spectrograph as to the inclination of the orbital plane, and which should follow as closely as possible the methods and notation of cometary orbits. The following system is respectfully submitted. In giving it final form, Messrs. Hussey and Aitken, who have been especially interested in this subject, made several suggestions which have been incorporated. For the convenience of spectrographic and micrometric observers, the system is published both here and in Dr. Aitken's paper (*L. O. Bulletin* No. 71).

Let

P = the period of revolution, in mean solar years.

The elements P, T, e, and a have their usual significance. The elements Ω and ω have the significance attached to Ω and λ ($=\pi - \Omega$), respectively, by Burnham, See, Hussey, Aitken, and others.

T = the Greenwich mean time of periastron passage.

e = the eccentricity.

a = the major semi-axis of the orbit, in seconds of arc.

Ω = the position angle of that nodal point which lies between position angles 0° and 180°; that is, the position angle of the line of intersection of the orbit plane with the plane at right angles to the line of sight.

ω = the angular distance of periastron from the nodal point (Ω), measured along the orbit in the direction of the secondary's motion from 0° up to 360°. It should be stated whether the position angles are increasing or decreasing.[1]

i = the inclination of the orbit plane; that is, the angle between the plane at right angles to the line of sight and the orbit plane. Its value lies between 0° and ± 90°. i is positive, that is, between 0° and +90°, when the orbital motion at the nodal point (Ω) is away from the observer; and i is negative, that is, between 0° and −90°, when the orbital motion at the nodal point (Ω) is toward the observer.[2]

μ = the mean daily motion of the companion measured in the direction of motion.[3]

There is apparently no simple system of defining the elements of stellar orbits which will apply equally well to both visual and spectroscopic orbits. The difficulty lies in the following facts. The computer of the visual orbit must publish his elements with the quadrant of i left unknown: and the computer of the spectrographic orbit must publish his elements with the value of i completely unknown. In the former case the micrometer leaves the position angle of the ascending node and the distance from the ascending node to the periastron both uncertain by 180°; the spectrograph determines which is the ascending node, but to change the visual elements Ω and ω by 180° would introduce confusion. It is preferable simply to retain

[1] The direction of the motion being described in the elements, it becomes possible to construct the orbit from the elements alone, and without recourse to the observations.

[2] The inclination determined from micrometer measures alone should always carry the double sign ± (plus *or* minus), for the sign of the inclination is indeterminate. As soon as relative radial velocities have been measured with the spectrograph, only one sign of i need be retained; and no other change in the elements will be required in order to define the orbit completely.

[3] The formulæ used in computing the apparent position angle θ and the apparent distance ρ at the time t, from the elements given on this system are

$$M = \mu\,(t-T) = E - e\sin E$$

$$r = a\,(1 - e\cos E)$$

$$\tan\frac{v}{2} = \sqrt{\frac{1+e}{1-e}}\tan\frac{E}{2}$$

$$\tan(\theta - \Omega) = \pm\tan(v+\omega)\cos i \quad \left[\pm \text{ according as position angles are } \begin{matrix}\text{increasing}\\ \text{decreasing}\end{matrix}\right]$$

$$\rho = r\,\frac{\cos(v+\omega)}{\cos(\theta-\Omega)}\,.$$

In the case of increasing position angles, $\theta - \Omega$ and $v+\omega$ are in the same quadrant; but in the case of decreasing position angles they are in different quadrants.

the correct sign of i and drop the incorrect one. In the spectrographic orbit, the observations determine the longitude of periastron (ω) measured from the ascending node. In a determination of the elements of the same orbit micrometrically, the value of ω may differ 180° from the spectrographic ω. To equalize them by changing the spectrographic system of nomenclature would likewise introduce confusion. It seems wise to use the double star system of nomenclature described above in dealing with visual orbits, and the nomenclature of Lehmann-Filhés' method[1] of determining spectrographic orbits in the other case. Transformation from one system to the other will be easy for any given orbit determined from the two classes of observations.

The parallax and the elements of the orbits of the binary star *Sirius* are probably more accurately determined than in the case of any other double star, with the possible exception of *α Centauri*. Zwiers' elements on the above system are:[2]

$P = 48.8421$ years
$T = 1894.0900$
$a = 7''.594$
$e = 0.5875$
$\Omega = 44°\ 30'.2$ (1900.0)
$\omega = 147\ 53.6$, position angles decreasing
$i = \pm 46°\ 1'.9$
$\mu = 7°.37069$

The weighted mean value of the Cape of Good Hope determinations of the parallax is $0''.37$.[3] Auwer's[4] result for the relative masses of *Sirius* and its companion is 2.20:1.04.

Lehmann-Filhes[5] has developed a very convenient formula for determining the radial velocity of a star due to its orbital motion. If we let the radial velocity V be expressed in kilometers per second, a in seconds of arc, P in mean solar years, π'' the star's parallax in seconds of arc, 149,500,000 the mean distance of the Sun in kilometers corresponding to a solar parallax of $8''.80$, then

[1] *Astronomische Nachrichten*, **136**, 16, 1894.

[2] *Proceedings of the Amsterdam Academy of Sciences*, May 27, 1899.

[3] Sir David Gill, *Monthly Notices, R. A. S.*, **58**, 81, 1898.

[4] *Astronomische Nachrichten*, **129**, 232, 1892.

[5] *Ibid.*, **136**, 19, 1894, Equation (2).

$$V=\frac{149{,}500{,}000\,a\cdot 2\pi\sin i}{365.25\cdot 86{,}400\,\pi''\,P\sqrt{1-e^2}}[e\cos\omega+\cos(v+\omega)]\ ,$$

or,
$$V=[1^{*}.47372]\frac{a\sin i}{\pi''\,P\sqrt{1-e^2}}[e\cos\omega+\cos(v+\omega)]\ .$$

In the spectrographically-determined orbit, i is always plus, for the deduced value of ω is measured from the ascending node. Care must be taken to distinguish between the motions of the companion with reference to the primary, and of the primary and secondary with reference to the center of mass of the system.

The value of V for the companion of *Sirius* with reference to the primary becomes

$$V=\mp 5.536\pm 11.125\cos(v+147^{\circ}\ 53'.6)\ .$$

It will appear in the sequel, from the observations with the Mills spectrograph, that i (in the visual orbit) is plus. Therefore the radial velocities of the companion with reference to the primary are given by

$$V=-5.536+11.125\cos(v+147^{\circ}\ 53'.6)\ .$$

It follows, from Auwer's values of the relative masses, that the radial velocities of the secondary with reference to the center of mass of the system are given by

$$V_2=\frac{2.20}{3.24}[-5.536+11.125\cos(v+147^{\circ}\ 53'.6)]\ ,$$
$$V_2=-3.759+7.554\cos(v+147^{\circ}\ 53'.6)\ ;$$

and the radial velocities of the primary with reference to the center of mass of the system are given by

$$V_1=-\frac{1.04}{3.24}[-5.536+11.125\cos(v+147^{\circ}\ 53'.6)]\ ,$$
$$V_1=+1.777-3.571\cos(v+147^{\circ}\ 53'.6)\ .$$

The values of V, V_2, and V_1 are tabulated for the convenience of other observers at intervals of one year throughout the period of a revolution, working each way from $T=1894.0900$, to the time of apastron passage, 1918.5110. The maximum relative velocities of approach and recession occur at the two nodal points, and the extreme range of the primary's speed is the arithmetical sum of these maxima, or $5.35+1.79=7.14$ km.

* Logarithm of factor constant for all orbits.

	V	V_2	V_1		V	V_2	V_1
1870.09	+3.99	+2.71	−1.28	1894.09	−14.97	−10.16	+4.81
71.09	4.21	2.86	1.35	95.09	16.66	11.31	5.35
72.09	4.42	3.00	1.42	96.09.	15.32	10.40	4.92
73.09	4.63	3.15	1.48	97.09	12.96	8.79	4.17
74.09	4.82	3.27	1.55	98.09	10.63	7.21	3.42
75.09	4.99	3.38	1.61	99.09	8.65	5.87	2.78
76.09	5.14	3.49	1.65	1900.09	6.98	4.74	2.24
77.09	5.28	3.58	1.70	01.09	5.59	3.79	1.80
78.09	5.40	3.66	1.74	02.09	4.40	2.99	1.41
79.09	5.49	3.72	1.77	03.09	3.38	2.30	1.08
80.09	5.56	3.77	1.79	04.09	2.50	1.70	0.80
81.09	5.59	3.79	1.79	05.09	1.71	1.16	0.55
82.09	5.59	3.79	1.79	06.09	1.02	0.69	0.33
83.09	5.54	3.76	1.78	07.09	−0.41	−0.28	+0.13
84.09	5.43	3.68	1.75	08.09	+0.41	+0.10	−0.04
85.09	5.24	3.55	1.69	09.09	0.65	0.44	0.21
86.09	4.93	3.34	1.59	10.09	1.11	0.75	0.36
87.09	4.47	3.04	1.43	11.09	1.54	1.05	0.49
88.09	3.77	2.56	1.21	12.09	1.94	1.32	0.62
89.09	2.74	1.86	0.88	13.09	2.30	1.56	0.74
90.09	+1.15	+0.78	−0.37	14.09	2.64	1.80	0.84
91.09	−1.29	−0.88	+0.41	15.09	2.95	2.01	0.94
92.09	5.02	3.41	1.61	16.09	3.25	2.21	1.04
93.09	10.13	6.87	3.26	17.09	3.53	2.40	1.13
94.09	−14.97	−10.16	+4.81	18.09	3.78	2.57	1.21
				1918.511	+3.86	+2.62	−1.24

The foregoing computations were made, for the most part, shortly after the publication of Zwiers' orbit. Doberck has recently computed an orbit[1] of *Sirius*, using observations up to 1903.16. However, his elements differ very little from Zwiers', and the two orbits give substantially the same radial velocities. The computed total range in the primary's radial velocity from Doberck's orbit is 6.61 km, as against 7.14 km from Zwiers' orbit.

Thirty-one spectrograms of the bright component of *Sirius* have been obtained with the Mills spectrograph since the year 1896. Mr. Burns has recently made definitive measures of all of them, and his results are given below. The negatives of 1896 and 1897 were obtained with the original imperfect camera lens referred to in the published description[2] of the spectrograph. The field of good definition for this lens was a point, and the field of moderately good definition was very small. In the Sirian type of spectrum there were therefore very few lines suitable for safe measurement, and the

[1] *Astronomische Nachrichten*, **166**, 321–326, 1904.

[2] *Astrophysical Journal*, **8**, 132–134, 1898.

results have smaller weight than those secured later with more perfect lenses. The lines in this type of spectrum are rendered difficult of measurement both by underexposure and by overexposure, and that exposure time which gives the proper density should be determined by experiment. This has not been practicable in the present series of observations, as they were obtained under a great variety of conditions, for the most part incident to the experimental development of the subject; with five different camera lenses having three quite different focal lengths; without and with a correcting lens in front of the slit; with straight and curved slits; with two different spectrographs; on slow, medium, and quick plates; under various atmospheric conditions; and by several observers. The correct exposure time for this brilliant star is very short; and, in the observer's solicitude to distribute the light uniformly along the slit, the negatives were overexposed in many cases. All the spectrograms are included in the following list; none have been rejected. The accordance of the results for any given epoch could easily be improved by making the series of observations under uniform conditions and with properly timed exposures.

Neglecting the plate of 1898.07, which is very poor and stands alone, there is an unmistakable progression in the results, which we attribute to the effect of orbital motion. Whether the irregularities in the progression are real, and due to unrecognized disturbing forces in the system, or are purely accidental, cannot now be stated; but they should be examined in connection with future series of observations.

The observed progression is in the direction of algebraically decreasing velocities, and this determines that the positive value of the inclination i is to be used.

The observed velocity should equal the computed orbital velocity plus the velocity of the center of mass of the system. If we let V_m represent the velocity of the center of mass, then each observation supplies an equation of the form

$$V_m = V_{\text{observed}} - V_1 \ .$$

Combining the thirty-one equations, we obtain as the velocity of the system of *Sirius*,

$$V_m = -7.36 \text{ km per second} \ .$$

Date	Velocity	Remarks
1896, October 2.....	−1.4 km	Overexposed
October 2.....	−1.0	Slightly overexposed
October 3.....	−1.6	Badly overexposed
1897, January 5.....	−3.6	Lantern-slide plate
January 21.....	−4.3	
February 22.....	−5.6	
February 23.....	−5.3	Underexposed, lantern-slide plate
February 24.....	−3.2	Lantern-slide plate
Mean 1896.97, 8 plates....	−3.2 km	
1898.07, 1 plate.....	−5.9 km	Badly overexposed
1898, September 19.....	−3.8	32-inch camera, comparison poor
September 22.....	−5.0	Comparison incomplete on one side
September 28.....	−3.7	Seed 23 plate
October 9.....	−2.1	Seed 23 plate, comparison poor on one side
Mean 1898.74. 4 plates....	−3.6 km	
1899, September 26.....	−5.0	
October 23.....	−4.2	32-inch camera
October 23.....	−3.3	32-inch camera
October 23.....	−4.3	32-inch camera
1900, February 14.....	−5.7	32-inch camera
March 20.....	−5.6	32-inch camera
Mean 1899.92, 6 plates...	−4.8 km	
1901.93, 1 plate	−4.8 km	
1902, December 1.....	−6.6	Star poor, comparison very poor
December 17.....	−7.4	
December 22.....	−7.0	Poor plate
1903, February 10.....	−6.2	
February 10.....	−5.8	Comparison poor on one side
February 10.....	−7.2	Comparison poor
February 15.....	−7.3	Overexposed, focus poor
February 23.....	−6.5	Overexposed
February 23.....	−7.8	Focus rather poor
Mean 1903.07, 9 plates....	−6.9 km	
1904, December 13.....	−5.6	Overexposed
December 13.....	−5.2	
Mean, 1904.95, 2 plates...	−5.4 km	
1905, February 13*.....	−7.8	Overexposed
February 13.....	−7.0	Overexposed
Mean, 1905.12, 2 plates...	−7.4 km	

*Added while passing through the press.

Spectrographic observations of *Sirius* have been made, and published, by Vogel and Scheiner[1] at Potsdam, by Deslandres[2] at Paris, and by Adams[3] at the Yerkes Observatory.

Vogel and Scheiner secured ten spectrograms from 1888 to 1891. Their results were quite accordant, but they are not in harmony with the velocities required by the recent elements of *Sirius*' visual orbit and the value $V_m = -7.36$, just determined.

Deslandres' 1891 observation lies on the opposite side of these requirements, but his 1895 observation lies on the same side and near to them.

Adams' excellent series of ten observations obtained from December 1901 to March 1902 is in good agreement with the Mount Hamilton results. However, the combination of their results with the Potsdam velocity for 1890.09 could not be expected to give a value of the parallax approximating to Gill's value, as the computed difference in the velocities at the two epochs is only 1.79 kilometers.

The following table contains all the published observations known to me, combined into groups, those forming each group covering only a short interval of time. The number of observations in each group

Observations Made at	Date	No. of Observations Combined	Observed Velocity	V_1	V_1+V_m
Potsdam.....	1888.99	3	−13.9 km	−0.92 km	−8.3 km
Potsdam.....	1890.09	3	−17.0	−0.37	−7.7
Paris.....	1891.17	1	−1.2	+0.49	−6.9
Potsdam.....	1891.20	4	−14.9	+0.52	−6.8
Paris.....	1895.21	1	−4.1	+5.30	−2.1
Lick.....	1896.97	8	−3.2	+4.26	−3.1
Lick.....	1898.07	1	[−5.9]	+3.43	−3.9
Lick.....	1898.74	4	−3.6	+2.99	−4.4
Lick.....	1899.92	6	−4.8	+2.33	−5.0
Lick.....	1901.93	1	−4.8	+1.47	−5.9
Yerkes.....	1902.06	10	−6.9	+1.42	−5.9
Lick.....	1903.07	9	−6.9	+1.09	−6.3
Lick.....	1904.95	2	−5.4	+0.58	−6.8
Lick.....	1905.12	2	−7.4	+0.54	−6.9

[1] *Publicationen des Astrophysikalischen Observatoriums*, **7** (I), 97, 101, 1892; also *Astronomy and Astro-Physics*, **11**, 151–57, 1892.

[2] *Comptes Rendus*, **113**, 739, 1891; also *Spécimens de photographies astronomiques*, Paris, 1897.

[3] *Astrophysical Journal*, **15**, 215, 1902.

is indicated in column three. The computed relative orbital velocity, V_1, of the primary is given in column 5. The last column contains the corresponding values of

$$V_1+V_m=V_1-7.4 \text{ km} .$$

LICK OBSERVATORY,
UNIVERSITY OF CALIFORNIA,
January 25, 1905.

A LIST OF NINE STARS WHOSE RADIAL VELOCITIES VARY

By W. W. CAMPBELL and HEBER D. CURTIS

The following nine spectroscopic binary stars discovered from observations made with the Mills spectrograph are additional to the forty-eight[1] already announced. Some of the stars on the present list have been suspected of variability for several years, and two of the stars, *α Andromedae* and *τ Sagittarii*, were definitely known to be binary here in advance of their prior announcement by other observers.

Measures given to tenths of a kilometer are definitive, and those to the nearest kilometer are preliminary and approximate unless otherwise stated.

α Andromedae ($\alpha = 0^h\ 3^m$; $\delta = +28°\ 33'$)

This star is of Type A, with a good magnesium line at λ 4481. It was announced as a binary by Slipher in *Lowell Observatory Bulletin* No. 11.

The Lick Observatory measures are as follows:

Date	Velocity	Observer
1901, August 13	−18km	Reese
	−17	Campbell
1903, September 22	−26	Curtis
September 27	− 8	Curtis
	−10	Brasch[2]
October 5	− 2	Brasch
October 12	− 6	Curtis
	− 4	Brasch
November 30	−36	Curtis
	−35	Brasch
December 21	−28	Moore[2]
	−26	Brasch

The binary character of this star was discovered from the third plate by Dr. H. D. Curtis.

ζ Ceti ($\alpha = 1^h\ 46^m.5$; $\delta = -10°\ 50'$)

[1] This does not include the five binaries announced by Wright in *Bulletin* No. 60, discovered by the D. O. Mills Expedition to Chile; nor the variable stars W *Sagittarii*, Y *Sagittarii*, and S *Sagittae*, discovered to be spectroscopic binaries by Dr. R. H. Curtiss, using the one-prism spectrograph, as announced in *Bulletin* No. 62.

This star is of Type K, with good lines. Though the observed variation is small, there seems to be no doubt of its reality. The period is doubtless several years. The variable velocity was suspected from the third plate by Director Campbell.

Date	Velocity	Observer
1897, November 25	+10km	Campbell
	+10.9	Burns[1]
1898, October 11	+ 9	Campbell
	+6.2	Burns
November 14	+ 5	Campbell
	+ 3.8	Burns
1899, August 14	+ 6	Campbell
	+ 6.0	Burns
August 22	+ 6	Campbell
	+ 5.5	Burns
September 26	+ 7	Campbell
	+ 6.7	Burns
1900, August 12	+ 7.6	Burns
1901, August 14	+ 8.0	Burns
September 1	+10.2	Burns
1903, September 15	+ 7	Curtis
1904, September 7	+ 9	Curtis

γ Geminorum ($\alpha = 6^h\ 31^m.9$; $\delta = +16°\ 29'$)

This star's spectrum is of the Sirian type, and the lines are capable of accurate measurement. The variable velocity was discovered by Mr. Burns, from the definitive measures of the third plate.

Date	Velocity	Observer
1899, September 21	−17km	Campbell
	−15.4	Burns
October 24	−17	Campbell
	−15.1	Burns
1904, January 27	− 4.7	Burns
February 13*	−10.4	Burns

α^2 *Geminorum* ($\alpha = 7^h\ 28^m$; $\delta = +32°\ 7'$)

A spectroscopic binary of unusual interest was discovered in α^2 ***Geminorum*** by Dr. Heber D. Curtis in October 1904. Twenty-five plates secured to date with the Mills spectrograph show a variation in the radial velocity of about 26 km. The observations seem to be

[1] Dr. Joseph H. Moore and Mr. Keiven Burns are Carnegie Institution assistants in spectroscopy. Mr. F. E. Brasch was for a short time the Carnegie Institution computer in this department.

*Added while passing through press.

well satisfied with a period of 9.27 days. Two early plates of this component of *Castor* taken on November 18, 1897, and March 31, 1901, are unfortunately rather poor, so that their value in a more accurate determination of the period is somewhat impaired, and additional plates will be needed before this period can be regarded as definitely established.

As is well known, a^1 *Geminorum*, the fainter component, was found to be a spectroscopic binary by Dr. Bélopolsky at Pulkowa, with a period of 2.934 days.[1]

Both stars are given in the *Draper Catalogue* as of Type A, and in the later Harvard classification as VIII*a*. In the region covered by the remounted Mills spectrograph, λ 4500 central, the spectra of both components are precise duplicates of that of *Sirius*. *Hγ* is broad and has not been used in the measures; the magnesium line λ 4481 is very good, and there are numerous other metallic lines, mainly enhanced lines of iron and titanium, with a few apparently due to chromium and barium.

The discovery is of special interest in that *Castor* is thus definitely shown to be a quadruple system. As is well known, *Castor* is one of the most conspicuous of the visual binaries. Its orbit is still somewhat uncertain; the latest and most probable elements are by Doberck,[2] giving a period of 347 years. The preliminary value of the velocity of the center of mass of a^2 *Geminorum* is about +6 km. For a^1 *Geminorum* a corresponding value of −2 km has been derived from a curve depending upon nineteen plates. Applying this relative velocity to Doberck's orbit, we find a parallax of 0".05; but, owing to the uncertainty in the elements of the visual system, this result is of small weight.

Dr. Curtis has in progress a more detailed investigation of the orbits of both systems. With the improved values which will eventually be found for the visual system it should be possible to secure relatively accurate values of the distance, masses, and orbital dimensions of this complicated system.

[1] *Bulletin of the St. Petersburg Academy of Sciences*, December 1896; *Astrophysical Journal*, **5**, 1, 1897.

[2] *Astronomische Nachrichten*, **166**, 145, 1904.

The annexed table gives the results of Dr. Curtis' measurements of the plates secured to date.

Date	Velocity	Remarks
1897, November 18.975.....	+ 1.1[km]	Poor
1901, March 31.740.....	+17.3	Very poor
1904, September 28.025.....	+ 1.6	
November 9.053.....	+18.9	
9.913.....	+15.9	
22.015.....	+ 4.4	
22.791.....	+ 0.9	
23.056.....	− 1.4	
29.025.....	+11.3	
29.819.....	+ 9.5	
December 6.025.....	+19.0	
7.042.....	+17.4	
8.049.....	+12.7	Not very good
13.986.....	− 3.0	
26.941.....	+13.5	
27.915.....	+ 9.3	
1905, January 1.955.....	+ 9.5	
2.652.....	+18.2	Very weak
2.994.....	+17.2	
3.951.....	+16.1	
4.938.....	+11.2	
9.875.....	− 8.5	
10.072.....	− 9.0	
10.997.....	+ 5.8	
11.049.....	+ 6.4	

η Boötis ($\alpha = 13^h\ 49^m.9$; $\delta = +18°\ 54'$)

This star is type G, with very good lines.

Date	Velocity	Observer
1897, February 3.....	− 0.6[km]	Campbell
April 20.....	− 2	Reese
21.....	− 4	Reese
May 12.....	− 2	Campbell
1899. February 7.....	− 2.2	Stebbins
1901, May 19.....	− 4	Reese
	− 4.9	Stebbins
1903, May 24.....	+ 7	Curtis
	+ 6.9	Burns
1904, March 2.....	− 8.2	Moore
March 31.....	− 4.9	Moore
May 2.....	− 6	Curtis
	− 7.6	Moore
June 14.....	−10	Curtis
26.....	− 6	Curtis
	− 7.3	Moore
1905, January 4.....	+ 5.5	Moore

The period is probably several years. The variable velocity was discovered by Dr. Moore.

ξ *Serpentis* ($\alpha = 17^h\ 32^m$; $\delta = -15°\ 20'$)

This star is given in the *Draper Catalogue* as Type A. Its spectrum contains many metallic lines, and in general resembles that of *Sirius*.

Date	Velocity	Observer
1902, July 1	−45.0km	Burns
1903, June 22	−62	Curtis
	−60.3	Moore
1904, May 30	−39	Curtis, Underexposed
	−39.2	Moore
June 13	−49	Curtis
	−49.3	Moore
June 14	−41	Curtis
	−42.7	Moore

The period is probably short. The variation was discovered by Dr. H. D. Curtis from the third plate.

ζ *Lyrae*, pr. ($\alpha = 18^h\ 41^m.3$; $\delta = +37°\ 30'$)

This star is of Type A; it has numerous titanium lines and enhanced iron lines.

Date	Velocity	Observer
1902, July 15	+21km	Curtis
1903, July 21	−31	Curtis
1904, May 15	− 5	Curtis
August 1	+22	Moore

The variation was discovered by Dr. H. D. Curtis from the second plate.

τ *Sagittarii* ($\alpha = 19^h\ 0^m.7$; $\delta = -27°\ 49'$)

This star is of Type K, with very good lines. Its variability was suspected by Dr. Curtis in May 1903, and confirmed in October 1903. It has already been announced by Wright in *Lick Observatory Bulletin* No. 60 as one of the results of the D. O. Mills Expedition to the Southern Hemisphere.

Date	Velocity	Observer
1902, August 17	+34km	Stebbins
1903, May 6	+50	Curtis. Poor plate
October 25	+24	Curtis
1904, May 12	+51	Palmer
June 7	+60	Palmer

The last two plates were taken at the Southern Station.

71 *Aquilae* ($\alpha = 20^h\ 33^m$; $\delta = -1^\circ\ 27'$)

This star is of type H, with very fair lines.

Date	Velocity	Observer
1900, June 18	− 2km	Wright
July 10	− 5	Wright
1903, September 22	+ 2	Curtis
	+ 3.0	Brasch
1904, August 30	−11	Curtis
	−11.3	Brasch
August 7	−12	Curtis
	−12.7	Brasch

The variable velocity was discovered by Dr. H. D. Curtis from the third plate.

LICK OBSERVATORY,
UNIVERSITY OF CALIFORNIA
January 25, 1905.

ON THE RADIAL VELOCITIES OF *POLARIS*, *η PISCIUM*, *ε AURIGAE*, AND *β ORIONIS*

By W. W. CAMPBELL and HEBER D. CURTIS

α Ursae Minoris ($\alpha = 1^h\ 22^m.0$; $\delta = +88°\ 46'$)

Groups of plates of *Polaris* taken at intervals during the past four years show that the velocity of the center of mass of the rapid pair of this triple system is changing in a very regular manner along a velocity-curve which indicates a period of at least eleven or twelve years. The period may be considerably longer than this, but the observations do not as yet permit even an approximate determination of its length. The accompanying table gives the positions of the *minima* of the velocity-curve of short period (3 days, 23 hours, 14 minutes) at the dates indicated. These values have been determined graphically; and it is to be expected that a definitive discussion of the data will change them slightly.

Date	Positions of Minima
1896.9	−20.67 km
1899.8	−14.22
1900.6	−14.64
1901.4	−16.32
1902.6	−16.79
1903.0	−17.18
1903.7	−17.84
1904.5	−18.52

The position of the series of 1901.4 falls nearly a kilometer off the curve; the agreement of the others is very close.

η Piscium ($\alpha = 1^h\ 26^m.2$; $\delta = +14°\ 50'$)

In the *Astrophysical Journal* for May 1904 Professor Lord has published eleven radial velocities of *η Piscium*. They range from +9.5 to +25.4 km, and he suspects that the velocity varies in a long period.

The following observations have been secured at Mount Hamilton:

Date	Velocity	Observer	Remarks
1897, September 14..... ...	+16.6	Burns	
1898, August 19.....	+16.5	Burns	
August 29.....	+15.8	Burns	
October 3.....	+13.3	Burns	Plate overexposed
1904, July 6.....	+15.6	Curtis	
July 18.....	+16.1	Curtis	
October 17.....	+15.5	Curtis	

Professor Lord's plates were all secured between the dates December 15, 1901, and January 9, 1904, during which interval no observations were made here. Our results neither prove nor disprove that the star is a spectroscopic binary. Further observations are desirable before a decision can be made.

ε Aurigae ($\alpha = 4^h\ 54^m.8$; $\delta = +43°\ 41'$)

This star has been announced as a spectroscopic binary by Professor Vogel.[1] The first few plates of this star taken with the Mills spectrograph showed considerable variation, and it was placed on the "suspected list" early in 1900. Recent definitive measures of these plates fully confirm its binary character.

Date	Velocity	Observer	Remarks
1897, January 7.....	+ 9	Campbell	Very poor plate
1899, December 18.....	+ 4.2	Burns	
1899, December 27.....	+ 5.7	Burns	
1900, January 17.....	+10.0	Burns	
September 24.....	− 2.6	Burns	
1903, December 29.....	− 5	Moore	
	− 3	Brasch	

The period as pointed out by Professor Vogel is doubtless several years.

β Orionis ($\alpha = 5^h\ 9^m.7$; $\delta = -8°\ 19'$)

From fourteen Potsdam spectrograms of this star, made in the years 1888–1891, velocities ranging from +3 to +34 km per second were obtained, and Professor Vogel strongly inclined to the view that the velocity is variable.[2] Nineteen spectrograms by Frost and Adams at the Yerkes Observatory, between September 1901 and

[1] *Astrophysical Journal*, **17**, 243, 1893.

[2] *Potsdam Publications*, **7** (I), 146–151, 1892.

March 1902, give results ranging from +14.9 to +23.4 km, the mean value being +20.7 km, but these observers attribute the observed variations entirely to the difficulty of measuring the wide lines in this spectrum.[1]

The following observations have been secured at Mount Hamilton with the Mills spectrograph. Where two measures of a plate are given, the first is approximate and the second definitive.

Date	Velocity	Observer	Remarks
1897, October 6.....	+16 km	Campbell	
	+16	Burns	
December 15.....	+20	Campbell	Poor plate
	+25	Burns	
1900, February 5.....	+18	Campbell	
	+15	Burns	
1901, October 15.....	+20	Burns	
1904, January 26.....	+18	Moore	

The mean value of the definitive measures is +18.9 km. These photographs were taken without preliminary tests to determine what exposure time would define the lines most clearly; and, as a result, the negatives are for the most part overexposed, but none have been rejected. Considering the small number of lines available, and their poor quality, we may say that these results give no evidence of variable velocity. Properly timed exposures would, in our opinion, reduce the observed range appreciably.

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January 25, 1905.

[1] *Publications of the Yerkes Observatory*, **2**, 52–61, 1903.

Minor Contributions and Notes.

ON THE COMPARATIVE LUMINOSITY AND TOTAL RADIATION OF THE SOLAR CORONA

In my report of the bolometric measurements made at the eclipse of May 28, 1900,[1] I stated that the luminosity of the inner corona, as compared with its heating effect, is so great that it is difficult to attribute the coronal radiation to the incandescence of particles of matter made hot by their proximity to the Sun, or even to suppose that the corona shines chiefly by reflected photospheric rays.

Professor Arrhenius has recently controverted[2] the former inference on the ground that the supposed incandescent particles observed by the bolometer can be computed to maintain a temperature of about 4620° absolute, and that the spectrum of a black body at this temperature is sufficiently luminous in comparison with its total radiation to yield the results observed. Partly on account of an ambiguity of my statement of the position of the bolometer, Professor Arrhenius has computed the temperature only of the *hottest particle* which took part in the radiation observed, apparently without considering (*a*) that the farther edge of the bolometer strip was 1.2 mm instead of 0.2 mm from the image of the solar disk, and the ends of the strip more distant still; or (*b*) that all the particles along the line of sight from the nearest to the remotest limits of the corona took part in the radiation, as well as those situated immediately opposite the center of the Sun, so that the effective mean temperature of the supposed hot particles is lower than the temperature he has computed.

It follows from a recomputation that the mean effective temperature of the observed part of the corona is about 3000°, assuming that it is composed of "black body" particles. If, further, we assign the proper luminous effectiveness to the different spectral rays, allow for the absorption of our atmosphere, and assume that the coronal region in question is only of the same average brightness as the Moon, it would seem that, if its radiation is mainly due to the incandescence of particles heated to this temperature by proximity to the Sun, its heat should have been many times as great as

[1] *The 1900 Solar Eclipse Expedition of the Astrophysical Observatory of the Smithsonian Institution*, p. 26.

[2] *Lick Observatory Bulletin* No. 58; *Astrophysical Journal*, **20**, 224, October 1904.

was actually observed by the bolometer. An independent computation, based on the comparative light and heat of the Sun and Moon, yields confirmative results.

It still seems to me, then, that the results of the bolometric experiments are incompatible with the hypothesis of a corona radiating chiefly by incandescence.

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January 1905.

NOTE ON NARROW TRIPLETS IN THE SPECTRA OF CALCIUM AND STRONTIUM

In the course of a somewhat extended investigation of the spectra of the elements of the second column of Mendelejeff's table, the writer has made a series of grating photographs, extending from λ 8000 to λ 2200. The apparatus and method were the same as those used in the study of the lithium family.[1] A preliminary survey of these films was made some time ago, and the series in the spectrum of strontium, which was recently announced by Fowler,[2] was noticed, along with some other new lines. A precisely similar series was also noticed in the spectrum of calcium; its terms are shown in the accompanying table.

NARROW TRIPLET SERIES IN CALCIUM SPECTRUM

Wave-Length	Frequency Intervals
4586.12	
	21.20
4581.66	
	13.53
4578.82	
4098.82	
	21.30
4095.25	
	13.80
4092.93	
3876.2	
	22.00
3872.9	
	13.34
3870.9	
3754.2	
	23.4
3750.9	
	13.5
3749.0	
3678.5	
	22.2
3675.5	
	8.2
3674.4	

[1] *Astrophysical Journal*, **20**, 188, 1904. [2] *Ibid.*, **21**, 81, 1905.

The first two of these triplets were measured by Kayser and Runge; the last three have not, so far as the writer knows, been noticed before. The intensities of the lines decrease, as they should, from triplet to triplet, with decrease in wave-length. The measurements here given may not be very accurate, as the lines are both faint and diffuse; the values for the last triplet, especially, are likely to be considerably in error. These lines are best shown by a photograph taken of the arc between *copper* poles, moistened with calcium-chloride solution. Such an arrangement prevents the appearance of the carbon bands, which would otherwise obscure all faint lines in this region.

The first lines of these triplets satisfy the formula

$$1/\lambda = 28930.0 - \frac{109675}{(n+0.8902)^2+0.2461},$$

where n has the values 3, 4, 5, 6, and 7, and 109675 is a universal series constant. This formula calls for another term near λ 6180, but, though there are lines in that neighborhood, none can be found with the required separations.

Another triplet, given by Kayser and Runge, beginning at λ 5601.51, has a similar separation, but its lines are irregular both in position and relative intensities, and cannot be classified with the others, if the series is to be represented by any of the usual formulæ. There are still other triplets of nearly the same separations, beginning at $\lambda\lambda$ 5270.45, 4512.73, 4059.1, and 3844.0, of which the last two are probably new. This set looks, at first sight, as though it might form a companion series, but such is apparently not the case, though it comprises all the remaining narrow triplets of this spectrum that have so far been found. The strontium spectrum shows indications of a similar set of lines, but this is irregular in the same way as the calcium group, and still less complete.

The writer's measurements of the lines in Fowler's series in the strontium spectrum agree closely with those given by him, as do also those of King[1] for the triplet near λ 4087. A new term should be added to the series at λ 3867.3$\pm$0.2 (only the first line can be seen on the writer's photographs); the position for this line, calculated from the second formula given by Fowler, is λ 3867.46.

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January 1905.

[1] *Ibid.*, **18**, 129, 1903.